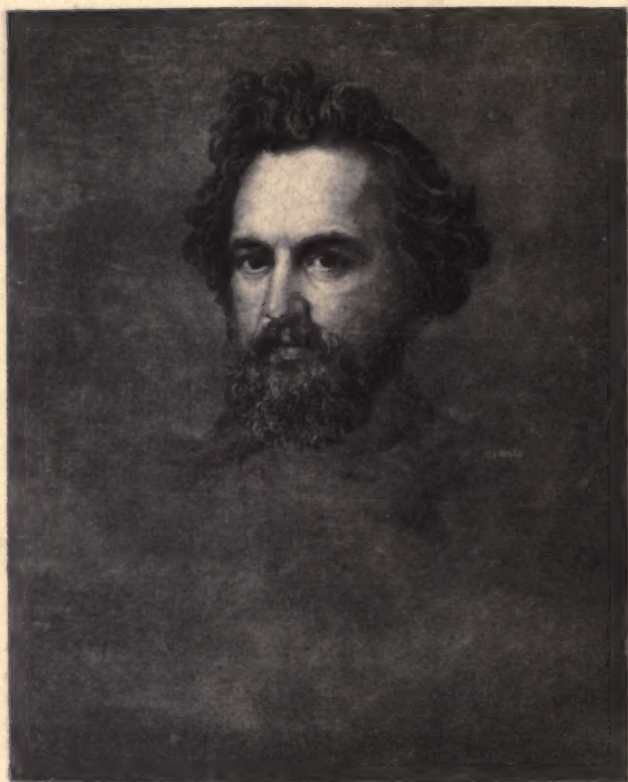




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G. F. Watts. pinx.

Frans Cornelis Donders

For Mem. RS

Born 27 May 1818 Died 24 March 1889

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PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

111

From December 11, 1890, to May 28, 1891.

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VOL. XLIX.

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PROCEEDINGS
OF
THE ROYAL SOCIETY.

December 11, 1890.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The President announced that he had appointed as Vice-Presidents—

The Treasurer.
The Astronomer Royal.
Professor Alfred Newton.
Sir G. Gabriel Stokes.
Lieut.-General Strachey.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On Ellipsoidal Harmonics." By W. D. NIVEN, F.R.S.
Received October 23, 1890.

(Abstract.)

In the paper, of which the following is an abstract containing statements of the principal results arrived at, an attempt has been made to develop the subject of ellipsoidal harmonics from their expressions in Cartesian coordinates.

The harmonics of the ellipsoid of three unequal axes are first investigated, as being the most readily dealt with on account of symmetry and, afterwards, those of the prolate and oblate spheroids are deduced as particular cases.

1. It has been found convenient to discuss separately the forms which are respectively suitable to the inside and outside of the ellipsoid, the former being taken first—

If Θ_r denote $\frac{x^2}{a^2 + \theta_r} + \frac{y^2}{b^2 + \theta_r} + \frac{z^2}{c^2 + \theta_r} - 1$,

the expressions comprised under

$$G = (1, x, y, z, yz, zx, xy, xyz) \Theta_1 \dots \Theta_n,$$

where any of the quantities inside the brackets is the multiplier of the product of the Θ 's outside, will satisfy Laplace's equation, provided n equations of the form

$$\frac{p}{a^2 + \theta_1} + \frac{q}{b^2 + \theta_1} + \frac{r}{c^2 + \theta_1} + \frac{4}{\theta_1 - \theta_2} + \dots + \frac{4}{\theta_1 - \theta_n} = 0$$

are satisfied, where p, q, r are respectively 3 or 1 according as G does or does not contain x, y, z as factors.

2. If K_r denote $\frac{x^2}{a^2 + \theta_r} + \frac{y^2}{b^2 + \theta_r} + \frac{z^2}{c^2 + \theta_r}$, so that $K_r = \Theta_r + 1$, then, in like manner, the expressions comprised under

$$H = (1, x, y, z, yz, zx, xy, xyz) K_1 \dots K_n$$

will satisfy Laplace's equation for precisely the same values of θ as in §1, and it may be shown that there are $2n+1$ independent conjugate H-harmonics of any degree n .

3. The function H is a spherical harmonic. Suppose it is of the n th degree and of order σ , and let it be denoted by H_n^σ . The corresponding ellipsoidal harmonic, *i.e.*, for the same values of θ , may be denoted by G_n^σ , and it may be shown that G_n^σ and H_n^σ are connected by the relation

$$G_n^\sigma = \left\{ 1 - \frac{D^2}{2(2n-1)} + \dots + (-1)^r \frac{D^{2r}}{2^r r! (2n-1)(2n-3) \dots (2n-2r+1)} + \dots \right\} H_n^\sigma,$$

where

$$D^2 = a^2 \frac{\partial^2}{\partial x^2} + b^2 \frac{\partial^2}{\partial y^2} + c^2 \frac{\partial^2}{\partial z^2}.$$

4. Let xyz be any point on the surface of the ellipsoid and $x'y'z'$ the corresponding point on a concentric sphere of unit radius, so that

$$x = ax', \quad y = by', \quad z = cz',$$

then will

$$\Theta_r(x, y, z) = -\theta_r K_r(x', y', z'),$$

and

$$G(x, y, z) = (1, a, b, \dots, abc) (-\theta_1) (-\theta_2) \dots H(x', y', z').$$

By means of these relations any function $f(x, y, z)$ or $f(ax', by', cz')$

can be first expressed in terms of spherical harmonics in x', y', z' , by Laplace's expansion, and then in ellipsoidal harmonics in x, y, z .

A series of ellipsoidal harmonics can thus be found having an arbitrary value at the surface of the ellipsoid.

5. *External Harmonics*.—The leading proposition in this part of the subject is as follows:—

If $\pi abc V_n$ denote the potential at an outside point xyz due to a solid ellipsoid, whose semi-axes are a, b, c , such that the density at any internal point fgh is of the form

$$n \left(1 - \frac{f^2}{a^2} - \frac{g^2}{b^2} - \frac{h^2}{c^2} \right)^{n-1},$$

then the harmonic of degree n and order σ , suitable to the space outside of the ellipsoid, is given by

$$G_n^\sigma I_n^\sigma = (-1)^n \frac{1}{2^n n!} H_n^\sigma \left(\frac{\partial}{\partial x}, \frac{\partial}{\partial y}, \frac{\partial}{\partial z} \right) V_n,$$

where
$$I_n^\sigma = \int_{\epsilon}^{\infty} \frac{d\lambda}{(\theta_1 - \lambda)^2 \dots \sqrt{(a^2 + \lambda)(b^2 + \lambda)(c^2 + \lambda)}}.$$

This result may primarily be regarded as a means of reducing the integral on the left-hand side, when the values of θ are known, into simpler forms, which can be actually evaluated when the surface is one of revolution. It is a result of some importance in the subject, as containing within itself the numerous expressions into which the external harmonics of spheroids can be thrown.

6. *Spheroids*.—The foregoing formulæ admit of easy reduction when two of the axes of the ellipsoid are equal, say $a = b$. It may then be shown that the spherical harmonics H of §2 are the ordinary spherical harmonic conjugate system. It is therefore convenient to adopt the definitions and specifications in Thomson and Tait's 'Natural Philosophy,' and thus to harmonise the spheroidal system with the spherical. Accordingly, if H be now used to express a spherical harmonic according to the definitions in that work, the new signification of G will be in accordance with the relation in §3.

Taking the results contained in §§3, 5, and effecting reductions suitable to the prolate spheroid, we obtain the following:—

$$G_n^\sigma =$$

$$j^\sigma \frac{2^{n-\sigma} (n+\sigma)! (n-\sigma)!}{(2n)!} \gamma^n \cdot 2 \cos \sigma \phi \cdot \frac{1}{\pi} \int_0^\pi P_n \left(\frac{z + j\rho \cos \theta}{\gamma} \right) \cos \sigma \nu \, d\theta,$$

$$I_n^\sigma = \frac{(2n)! (2n)!}{2^{2n} (n!)^3 (n-\sigma)!} \frac{1}{\gamma^{2n+1} \frac{d}{d\mu} P_n(\mu)} \int_{-1}^1 \frac{(1-v^2)^n}{(\mu-v)^{n+\sigma+1}} dv,$$

where

$$\gamma^2 + \epsilon = \mu^2 \gamma^2,$$

$$\gamma^2 = c^2 - a^2.$$

From these forms for G_n^σ and $G_n^\sigma I_n^\sigma$ a variety of others may be obtained, as well as expressions in the form of integrals for spherical harmonics of the second kind. Corresponding forms may also be established for oblate spheroids.

7. The expansion of the reciprocal of the distance between two points plays an important part in the application of these investigations. It has therefore been found in ellipsoidal harmonics and thence, by reduction, in harmonics of the spheroid, circular cylinder, and paraboloid of revolution, and its application has been briefly illustrated in finding the general term in the expansion of the potential due to the magnetism induced in an ellipsoid placed in any field of force, and in finding the electrical capacities of surfaces inverted from ellipsoids. In the same connexion, I have also found the expansion for the potential due to a thin shell bounded by similar and similarly situated ellipsoids, the density of which varies inversely as the cube of the distance from a fixed point.

8. In the last part of the paper I have shown how to prove what Heine terms "addition theorems" in the case of spheroidal harmonics, and thence, by reduction, in the case of Bessel's functions.

II. "Photometric Observations of the Sun and Sky." By WILLIAM BRENNAND. Communicated by C. B. CLARKE, F.R.S. Received October 30, 1890.

(Abstract.)

1. The paper begins with a short account of the various papers communicated by Sir H. Roscoe, and published in the Transactions of the Royal Society.

2. My observations were made at Dacca, East Bengal, in 1861-66, repeated at Milverton, in Somersetshire, during the last two years. My first experiments were directed to ascertaining the action of the sun on sensitised paper exposed at right angles to the solar rays for different altitudes of the sun, and largely to ascertaining the laws of distribution of the actinic power in the sky.

I take no observations except when the sky is quite clear.

3. The method of measurement I adopted is the darkening produced in sensitised paper. I cut strips from one uniform sheet of ordinary photographic paper. My observations being relative, I obtain the same results (ratios) with any paper. I compare ultimately the effects of the sun and of a candle on this same paper.

4. I assume that, in burning a stearine candle, the chemical action is proportional to the material consumed; I have taken as my unit (i)

of measure of chemical action the darkening produced at a distance of 1 inch from the wick of the candle when 100 grains were consumed, which in the candle I used in India occupied about forty-seven minutes. My observations, being almost entirely relative, are independent of these assumptions, which affect hardly any of my results except comparisons with the absolute unit measures of Sir H. Roscoe.

5. Explains the method by which I obtain a standard strip for the candle unit.

7. Describes the water motion actinometer, with which observations of the action of sun and sky were made.

8. Shows how it may be proved experimentally that the intensity of the action of light emanating from a physical point varies inversely as the square of the distance from the origin.

9. For obtaining the effects of the sun and sky, I have always experimented mainly by exposing the paper at right angles to the sun's rays. Sir H. Roscoe, on the other hand, exposes his paper on a horizontal plane. Theoretic considerations have led me to another method of observation (with the "octant" actinometer below) which gives directly the measure of the effect really desired.

A table is given of the first observations I made, which afterwards led to the formation of Table B (see next page).

11. The method of observing the action of the sun alone.

12. Observations taken near the horizon not to be depended upon.

13. Refers to the construction of Table B, and the extension of the table for altitudes of the sun beyond those observed.

14. Shows how the numbers of the table were obtained, by taking the inverse of the times required at each altitude for producing the darkening of the candle unit.

17. I found the chemical action of the sun, as far as my experiments went, the same at all hours of the day and at all seasons of the year. And in Somersetshire I got exactly the same chemical action of the sun as at Dacca.

18. Various observations had led me to suspect that the chemical action of the sky at the same moment was diverse in different parts of it. To investigate this suspicion, I designed an instrument which I call the Mitrailleuse Actinometer (fig. 2). I mount a number of similar cylindrical tubes in one plane in a semicircle, to the centre of which the axis of each tube is directed: one extremity of each tube lies in the circumference of the circle; the other extremities lie on a concentric circle of about one-half the radius. In the circumference of this smaller circle is a semicircular series of holes, against which a semicircular block carrying the sensitised paper is pressed by a screw. Each cylinder cuts out of the sky a circle of $8^{\circ} 28'$ angular diameter. One of the tubes near its top carries a small plate of wood

Table B.
Chemical Action of Sun and Sky.

Sun's altitude.	Sun alone.	Sky alone.	Sun and sky together.	Sun's altitude.	Sun alone.	Sky alone.	Sun and sky together.
1...	0·001	0·003	0·004	31...	0·110	0·064	0·174
2...	0·002	0·005	0·006	32...	0·113	0·064	0·178
3...	0·003	0·007	0·010	33...	0·116	0·065	0·181
4...	0·004	0·010	0·014	34...	0·118	0·065	0·184
5...	0·006	0·012	0·019	35...	0·121	0·066	0·188
6...	0·009	0·016	0·025	36...	0·124	0·066	0·190
7...	0·012	0·019	0·031	37...	0·126	0·067	0·193
8...	0·016	0·022	0·038	38...	0·129	0·067	0·196
9...	0·020	0·026	0·045	39...	0·131	0·068	0·199
10...	0·024	0·029	0·052	40...	0·133	0·068	0·201
11...	0·028	0·032	0·060	41...	0·135	0·069	0·204
12...	0·033	0·035	0·068	42...	0·137	0·069	0·206
13...	0·038	0·038	0·075	43...	0·138	0·069	0·208
14...	0·043	0·040	0·082	44...	0·141	0·069	0·210
15...	0·047	0·042	0·090	45...	0·143	0·070	0·213
16...	0·052	0·045	0·097				
17...	0·057	0·048	0·105				
18...	0·061	0·049	0·110				
19...	0·066	0·050	0·116				
20...	0·070	0·052	0·122	50...	0·150	0·071	0·221
21...	0·075	0·053	0·128	55...	0·157	0·072	0·229
22...	0·079	0·054	0·134	60...	0·162	0·073	0·225
23...	0·083	0·056	0·139	65...	0·166	0·073	0·239
24...	0·086	0·057	0·144	70...	0·170	0·073	0·243
25...	0·091	0·058	0·149	75...	0·172	0·074	0·246
26...	0·094	0·059	0·153	80...	0·173	0·074	0·248
27...	0·097	0·060	0·158	85...	0·175	0·074	0·249
28...	0·101	0·061	0·169	90...	0·175	0·074	9·249
29...	0·104	0·062	0·166				
30...	0·107	0·063	0·170				

N.B.—For sun altitudes 50° to 90° , the figures are not the result of direct observations; for sun altitudes 1° to 10° , the figures are less certain by reason of thin haze often present.

on which stands a style parallel to the tube, by means of which this particular tube can be brought in a line with the sun. By another motion the plane of the tubes can be adjusted to the plane of symmetry (or elsewhere).

[A vertical plane through the sun at any time divides the visible sky into two exactly similar portions. I call this the plane of symmetry.]

19. The observations (Table C) were taken 23rd December, 1864, at Dacca (among other similar observations taken in the same cold

Table C.
(Sun's Altitude = $42^{\circ} 28'$.)

Altitude of the axis of the barrel of the mitrailleuse.	Distance of axis of barrel from the sun = θ .	Observed chemical action during six minutes' exposure = i_{θ} .	Calculated value of i_{θ} from $i_{\alpha} = 0.12 \operatorname{cosec} \theta$.
10°	$-32^{\circ} 58'$	0.2	0.221
20	$-22^{\circ} 58'$	0.5	0.308
30	$-12^{\circ} 58'$	0.7	0.535
40	$-2^{\circ} 58'$	..	2.319
50	7 2	0.844	0.98
60	17 2	0.322	0.41
70	27 2	0.188	0.264
80	37 2	0.184	0.199
90	47 2	0.177	0.164
100	57 2	0.144	0.143
110	67 2	0.14	0.1304
120	77 2	0.128	0.123
130	87 2	0.122	0.12
140	97 2	0.12	0.121
150	107 2	0.128	0.126
160	117 2	0.136	0.135
170	127 2	0.136	0.156

weather) in the plane of symmetry. The barrels of the mitrailleuse were fixed 10° apart, the altitude of the sun being $42^{\circ} 28'$.

I give the table as an early observation that shows well that there is a point of minimum sky intensity at 90° from the sun. It also appears that if i_{α} be the intensity for the altitude α of the sun ($= 0.12$), then the intensity of the sky at a point θ° from the sun is given (roughly only according to this table) by the formula

$$i_{\alpha} \operatorname{cosec} \theta.$$

This observation was made in the plane of symmetry: it turns out that the value, $i_{\alpha} \operatorname{cosec} \theta$, gives the intensity very accurately, for any point, in any other *great circle*, whose distance from the sun is θ° measured on that circle.

20. For any altitude of the sun (α), the chemical action of the sky is a minimum at all points in a great circle, the plane of which is at right angles to the line joining its centre to the sun.

[This plane I call the plane of minimum intensity (i_{α}).]

As the whole of the mathematical developments of this paper are founded upon the law that at any point of the sky whose distance is θ° from the sun

$$\text{the intensity} = i_{\alpha} \operatorname{cosec} \theta,$$

I have been careful to verify it by numerous observations both at Dacca and in Somersetshire, and also to vary the observations in every way I could devise. Thus the mitrailleuse has been placed in the plane of minimum intensity. In this case all the barrels give the same reading for points not too near the horizon.

Next the mitrailleuse was placed in planes of great circles through the sun at various angles with the plane of symmetry, by turning it round the line joining one of its tubes with the sun the observed chemical actions agree well with

$$i_a \operatorname{cosec} \theta.$$

Next by means of stops I made the aperture of each barrel of the mitrailleuse to be

$$c \sin \theta,$$

where θ° is the distance of the axis of the barrel from the sun; this mitrailleuse being exposed, the barrel $c \sin \theta$ being directed to the sun, the circular darkened spots were found to be very accurately of the same depth.

Further, I calculated the *times* of exposure for a (particular) mitrailleuse with barrels of uniform aperture, which ought, on the law $i_a \operatorname{cosec} \theta$, to give a uniform tint. I exposed this mitrailleuse for these calculated times, first in the plane of symmetry, afterwards in a plane inclined to it at 62° ; the results agreed closely with my anticipation, and show $i_a \operatorname{cosec} \theta$ to be a very good approximation.

22. I have therefore made full use of the expression $i_a \operatorname{cosec} \theta$ for the chemical action of the light of the sky in a circle θ° from the sun (whose altitude is α). First, in the following proposition:—

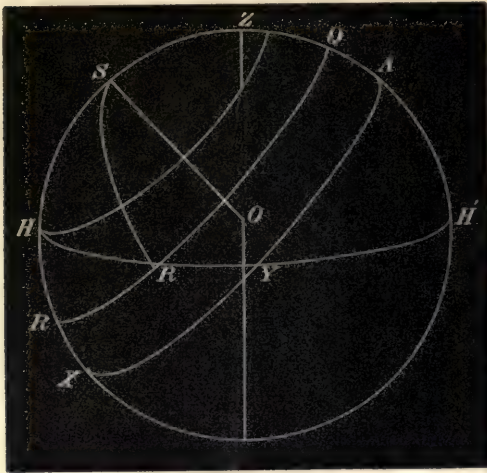
24. Having given i_a the chemical action in the circle of minimum intensity, to calculate the total chemical action of the sky on a plane exposed at right angles to the sun.

N.B.— i_a is a constant for this calculation, but it varies with the altitude of the sun.

Let the figure represent a projection on the plane of symmetry, S being the sun, Z the zenith, HRYH' the horizon, AYZ the plane of minimum intensity, SH = α the sun's altitude, θ the angular distance of the sun from QR. Then the total action of sky throughout the gore HYZSH on sensitised paper at O in the plane perpendicular to OS

$$= i_a \left\{ \pi + 2 \int_{\alpha}^{\frac{\pi}{2}} \frac{\tan \alpha \, d\theta}{\sqrt{(1 - \sec^2 \alpha \cos^2 \theta)}} \right\} \dots\dots\dots (K.)$$

The expression cannot be integrated; but, by using a formula of reduction in series, it gives—



Total intensity of the gore on the paper at O

$$= i_a \pi \left\{ 1 + \frac{2 \sin \alpha}{1 + \sin \alpha} \left[1 + 0.75 \tan^4 \left(\frac{\pi}{4} - \frac{\alpha}{2} \right) + 0.14 \tan^8 \left(\frac{\pi}{4} - \frac{\alpha}{2} \right) + \&c. \right] \right\},$$

which is the formula I have used in numerical computations.

It is the numerical value in the column "Sky alone" in Table B, which is thus brought into direct verification with i_a observed by the mitrailleuse.

Arts. 25—29 show that the integral (K) taken for the whole visible hemisphere is

$$2i_a (\pi \sin \alpha + 2 \cos \alpha) \dots\dots\dots (Q).$$

This is the whole chemical action of the hemisphere resolved on the horizontal plane, which was one of the quantities observed by Sir H. Roscoe.

30. Deals with any suspicion that may arise that the law of cosecants may have been assumed, the fact being that the law was arrived at, by experiment simply, more than twenty-two years ago, &c.

31. Applies the equation (Y) to determine i_a for the altitudes given by Sir H. Roscoe in his table showing the total chemical action of diffuse daylight (*i.e.*, of the whole sky, the sun being stopped off) on horizontally exposed paper ('Phil. Trans.,' 1870, p. 314). These values are tabulated with corresponding values of i_a calculated by formula in (24) from the Dacca Table B, forming together Table E.

32. As a first approximation from Table E, it would appear that Sir H. Roscoe's unit of chemical action is $\frac{1}{16}$ of the Dacca candle unit.

Table E.

1	2	3	4	5
Sun's altitude.	Diffused daylight of Roscoe.	i_a calculated from col. 2.	i_a calculated from Dacca Table B.	Values in col. 4 brought up for comparison with those in col. 3.
9° 51'	0·038	0·0076	0·0068	0·009
19 41	0·062	0·0105	0·0107	0·0141
31 14	0·100	0·0150	0·0118	0·0156
42 13	0·115	0·0160	0·0121	0·0160
53 9	0·126	0·0170	0·0121	0·0160
61 8	0·132	0·0177	0·0120	0·0159
64 14	0·138	0·0187	0·0120	0·0159

Twilight.

33. The resultant chemical action of the sky on a horizontally exposed piece of paper, the sun's altitude being α , is found

$$= (2\pi \sin \alpha + 4 \cos \alpha) i_a.$$

This vanishes when

$$2\pi \sin \alpha + 4 \cos \alpha = 0,$$

i.e., when

$$\tan \alpha = -\frac{2}{\pi},$$

or

$$\alpha = -32^\circ 29'.$$

This gives an absolute value for twilight, supposing daylight to cease when the diffused daylight of Roscoe entirely vanishes.

The extreme limit at which twilight has been certainly observed is when the sun is 24° below the horizon; at which time the formula $i_a(2\pi \sin \alpha + 4 \cos \alpha)$ would show the chemical action of diffuse daylight to be only $\frac{1}{40}$ of what it was just after sunset.

In other words, the formula

$$(2\pi \sin \alpha + 4 \cos \alpha) i_a$$

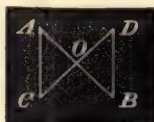
gives a very good agreement with the observed duration of twilight.

34. Taking as co-ordinate planes the plane of symmetry, the plane of minimum intensity, and the plane through the sun at right angles to these (which last I call the plane of the sun's altitude), it is found (as a corollary in Article 34) that [U], [V], [W], representing the total chemical effect of the sky, resolved on these co-ordinate planes.

This suggested the construction of the octant actinometer, which

requires only a quarter of the visible sky to be clear for observation, and gives the value of i_a directly, requiring no calculation or reduction. It possesses, moreover, the great advantage of not taking in the low band of sky near the horizon, and thus avoiding a principal element of uncertainty in other observations.

35. The octant actinometer consists of three quadrantal planes, MOS, MOI, and IOS, joined at their edges so as to form a hollow trihedral, and mounted so that one of the edges, OS, can be brought to point to the sun; the plane MOI will then coincide with the plane of minimum intensity. The instrument has another adjustment, by which it can turn round OS as an axis, and if one of the planes MOS, IOS be brought to coincide with the plane of symmetry, the other will coincide with the plane of the sun's altitude.



I take a small square of sensitised paper, and cut it along CO; then slipping the part COB under AOC, so that B coincides at C, it forms a rectangular trihedral of paper. This is placed in a small exposure trihedral of cardboard, and covered by a thin metal trihedral in the trihedral of the octant (I make several of these trihedrals of sensitised paper, so as in the field to take quickly a series of observations; the trihedral of paper is, of course, carefully covered till the instrument is in adjustment); exposed to the action of the sky for (say) thirty seconds, the readings on the planes MOS and IOS will be each $30i_a$, and that on the plane MOI will be $30 \cdot \frac{1}{2} \pi i_a$.

36. Gives in Table F the observations with the octant in August last.

37. Discussion regarding the most useful method of resolution of the sky and sun.

III. "Determinations of the Heat Capacity and Heat of Fusion of some Substances to test the Validity of Person's Absolute Zero." By SPENCER UMFREVILLE PICKERING, M.A., F.R.S. Received November 6, 1890.

The relations existing between the heat of fusion of a substance and its heat capacity in the liquid and solid condition were demonstrated by Person, in 1847 ('Ann. Chim. Phys.' (3), vol. 21, p. 315). He showed that the heat of fusion must diminish as the temperature

is lowered, the decrease per degree being equal to the difference between the heat capacities of the liquid and solid, and that, therefore, there must be a certain temperature at which the heat of fusion will be nil, this temperature being given by $t - \frac{l}{C - c}$, in which t is the melting point of the substance, l its heat of fusion at t , and C and c its heat capacity in the liquid and solid conditions respectively. At this temperature a liquid could not freeze, since there would be no difference between it and the solid, and Person argued that there would then be no heat at all in it, and that this temperature was the absolute zero. He then made determinations with various substances, which tended to show that this temperature was the same for all bodies, and was situated at -160°C .

The analogy between this zero and that deduced for gases is, however, very imperfect; the *total* heat in a gas is measured by its temperature reckoned from -273° , whereas it is only the *difference* between the total heat in a liquid and solid that is measured by its temperature reckoned from -160° , and, instead of considering the latter as the absolute zero, it is preferable to regard it as the critical temperature for the solid-liquid conditions (see 'Chem. Soc. Trans.,' 1889, p. 32); and indeed, since we have now succeeded in obtaining liquids at temperatures below -160° , it is quite impossible to regard -160° as the absolute zero, or to believe that the heat capacities of all bodies would indicate this same temperature for that of no solidification.

Guldberg ('*Bidrag til Agarnernes Molekylar Theorie*,' ch. v, p. 484) has defined the critical point of the solid-liquid states as that at which the volumes of the liquid and solid are identical, and at which the heat of fusion is nil, a certain pressure, as well as a certain temperature, being required to fulfil these conditions. It appears to me, however, that the question of pressure may practically be left out of consideration; pressure will, of course, affect the temperature in question, but to such a small extent that the ordinary atmospheric pressure, under which the data necessary for the calculations are obtained, may be regarded as nil, and it also appears to me that the definition depending on the heat of fusion being nil includes the idea of equal volumes, for it seems hardly possible to conceive two conditions of the same substance, each possessing the same kinetic and potential energy, which could yet differ from each other in volume or any other property.

The analogy, however, between this temperature and the critical temperature for the liquid gaseous conditions is at best but an imperfect one. If we start with a crystalline solid below this temperature and heat it, it could never pass by insensible degrees into a liquid; the molecules in a crystal possess a definite arrangement, those of a liquid an indefinite arrangement, and, between these two, no inter-

mediate state appears possible; on the other hand, when we start with a liquid and cool it, it becomes in many cases (see 'Chem. Soc. Trans.,' 1890, p. 340) so viscous that even at -80° to -100° it can scarcely be termed a liquid, and by further cooling it would probably become so firm that it would be regarded as a solid. This affords a striking illustration of what may be meant by the gradual passage of a liquid into a solid, but the solid thus obtained would evidently not be identical with the crystallised solid, nor could it be obtained by heating the crystals from a lower temperature; as the data on which the calculations are based do not refer to this solid but to the crystalline one, I think it preferable to avoid the use of "critical temperature," and to term that given by the equation $t - \frac{l}{C-c}$ the "temperature of no crystallisation."

For the purpose of extending the applications of a law governing the freezing points of solutions which I have lately propounded ('Chem. Soc. Proc.,' 1889, p. 149), it was necessary to determine this temperature in certain cases. The present communication contains the details of these determinations, and they afford evidence that it is not a constant for all bodies, as Person imagined.

The values which Person obtained with various substances were as follows:—*

Water	$-159^{\circ}7\ddagger$
Phosphorus	$-151^{\circ}7$
Sulphur	$-160^{\circ}3$
Sodium nitrate	$-156^{\circ}7$
Potassium nitrate	$-170^{\circ}9$
Hexahydrate of calcium chloride ..	$-165^{\circ}3$
Dodecahydrate of sodium phosphate	$-161^{\circ}0$
Potassium and sodium nitrate	$-161^{\circ}0$

The concordance of these values is certainly very striking, especially when the diversity of the substance examined and the difficulties of the determinations are considered; but a closer examination of the results cannot fail to suggest that the concordance must in some instances have been accidental. The difference between the heat capacities of the liquid and solid, $C-c$, is often very small, and even ordinary experimental errors in either of the quantities would make a large difference in the results, while in some of Person's determinations the experimental errors must have been of more than ordinary magnitude, for these determinations occasionally lasted between one and two hours, during which time the loss by cooling must have been very

* 'Ann. Chim. Phys.' (3), vol. 21, p. 295; vol. 24, p. 129; and vol. 27, p. 250.

† Taking Person's later determinations of the heat of fusion of water $-80^{\circ}0$ ('Ann. Chim. Phys.' (3), vol. 39, p. 73), this value becomes $-161^{\circ}3$.

large. Special sources of error and uncertainty might be pointed out in each particular case (except that of water, perhaps), but there is one fatal objection to all Person's results, namely, that the heat capacity of both solids and liquids varies considerably with the temperature, and that he made his determinations at any temperature which happened to be most convenient, that, for instance, at which the heat capacity of the solid was determined varying between 11° and 280° below its melting point.

One case will be sufficient to illustrate the effect of this. With solid phosphorus determinations have been made by Kopp ('*Liebig's Annalen*,' Suppl. 3, p. 1) and Regnault ('*Ann. Chim. Phys.*,' vol. 73, p. 56; and (3) vol. 26, p. 269), and the latter, at any rate, must command as much confidence as Person's (indeed Person adopts some of Regnault's determinations with this substance); these results are—

$$\begin{aligned} c &= 0.2020 \text{ at } (+36^{\circ} \text{ to } +13^{\circ} =) +24^{\circ}.5 \text{ C.} \dots \text{Kopp,} \\ c &= 0.1895 \text{ ,, } (+7^{\circ}.15 \text{ ,, } +30^{\circ}.21 =) +18^{\circ}.68 \text{ .. Regnault,} \\ c &= 0.1783 \text{ ,, } (-21^{\circ} \text{ ,, } +7^{\circ} =) -7^{\circ} \text{ .. Person,} \\ c &= 0.1699 \text{ ,, } (-77^{\circ}.75 \text{ ,, } +10^{\circ} =) -33^{\circ}.88 \text{ .. Regnault,} \end{aligned}$$

and, taken in their order, they give -1969° , -292° , -152° , and 102° for the temperature of no crystallisation,* which results clearly show that no value can be attached to Person's figure, -152° . Person's and Regnault's results lie in a fairly straight line which gives $c = 0.1979$ at the fusing point, and -719° as the temperature of no crystallisation, but it is impossible to accept even this value, as there are not sufficient data for calculating the heat capacity of the liquid at the fusing point.

C , c , and l should evidently be determined at the same temperature, and this temperature must necessarily be that of fusion (t), but, inasmuch as the heat capacities near this temperature may be abnormally high owing to the fusion or solidification being sometimes a gradual process, the determinations should not be made too near the fusing point,† and the only means of ascertaining their true value at this point is to determine them at several different temperatures, and from the rate of change thus obtained to calculate their value at the fusing point itself. This method has been adopted in the present work.

* $C = 0.2045$ (98° to 48°), $l = 5.034$, and $t = 44^{\circ}.2$.

† The values obtained by Person for beeswax afford a striking instance in point; from -9° to $+50^{\circ}$ the heat capacity of this solid increases from 0.43 to 1.72, exceeding at this latter temperature that of the liquid (0.50) by a very large amount. Ice shows a similar increase, but in a much smaller degree (see Person, '*Ann. Chim. Phys.*' (3), vol. 30, p. 80).

Substances Investigated.

The substances investigated were sulphuric acid, the monohydrate of sulphuric acid, the tetrahydrate of calcium nitrate, benzene, and naphthalene, the last mentioned being the only one in which C, c, and l had been determined by previous investigators. The sulphuric acid was the same as that used in my determinations of the freezing points of this substance ('Chem. Soc. Trans.,' 1890, p. 337), the stock acid having been diluted by the addition of ice so as to contain exactly 100, and, for the monohydrate, 84.488, per cent. H_2SO_4 .

The calcium nitrate was prepared by repeated crystallisation; the percentage of anhydrous salt in the fused crystals having been determined by evaporation and heating at 250° , the excess of water which the fused salt was found to contain was driven off by a gentle heat. The melted salt will remain liquid at ordinary temperatures for many days, although its solidifying point is $42^\circ.4$.

The naphthalene and benzene were special preparations made by Messrs. Kahlbaum: repeated crystallisation was not found to alter the melting point of either to any appreciable extent.

Method Employed.

The substance was placed in a cylindrical platinum bottle measuring 9×2 cm., and holding about 30 c.c. Its mouth was closed by a caoutchouc stopper, through which passed a thermometer with a very narrow bulb, long enough to extend from the top to the bottom of the bottle, thereby giving the mean temperature of the contents more accurately than an instrument with a short bulb would have done. The bottle was placed in a double test tube, and the latter in a double bath containing warm water or a freezing mixture, as the case might be. After the bottle had attained the required temperature, and this had remained constant for some time, it was removed from the test tubes and plunged into the calorimeter, an operation which occupied only two or three seconds. To prevent the deposition of hoar-frost on the bottle while it was being cooled, the inner test tube in which it was placed had a bulb blown at the bottom, in which was kept some sulphuric acid. The calorimeter contained either 600 or 1800 c.c. of water, the quantity being adjusted so that the rise or fall of the temperature in it was about 1° , the smallness of the change being favourable to the accuracy of the determination by rendering the loss of cooling, or gain by heating, very small. This loss or gain was estimated by determining the rate of cooling at the initial and final temperatures, both thermometers being read at intervals of one minute, and having been compared with each other before the determinations. The rate of cooling during the time when the temperature was rising or falling was taken to be the mean of

that at the initial and final temperatures; it was generally very small, since the temperature of the air was kept at a point such that there was heating at the initial temperature, and cooling at the final temperature, or *vice versa*. The time occupied in obtaining almost identical temperatures in the bottle and calorimeter varied between two and twenty minutes, the whole determinations, including the interval allowed for determining the two rates of cooling, occupying fifteen to forty-five minutes. Such a duration militates very much against the accuracy of the results.

The calorimetric thermometer read by estimation (0.05 mm.) to 0.0005; the stirring apparatus and other appliances were the same as those described elsewhere ('Chem. Soc. Trans.,' 1887, p. 293). The water equivalent of the platinum bottle and its thermometer was ascertained by direct experiment to be 2.223 grams. The last mentioned thermometer possessed a range of 30°, one estimation figure being equivalent to 0.01, and, as the rise or fall measured sometimes exceeded 30°, it was in such cases set so as to register the initial temperatures of the bottle, the final temperature of this being taken to be the same as that of the calorimetric water, previous determinations having shown that the two temperatures were identical within the reading error of the instruments when this rate of cooling became constant. The temperature of the calorimeter was generally about 18°.

The determinations were all made in duplicate. The mean error of a single observation was found to be about 0.8 per cent. of the total rise or fall measured; this corresponds to an error of 0.0075 in the alteration of temperature registered in the calorimeter, or 0.22 in that registered in the bottle; considering the long duration of the determinations and the magnitude of the total correction for cooling which had to be applied, such an error must, I think, be regarded as small. In many cases the error in the heat capacity found is the same as that in the rise or fall measured, *i.e.*, 0.8 per cent., of its value, or, on the average, 0.0032 of the heat capacity per gram; in other cases it is much greater, for the heat evolved sometimes included the heat of fusion, and, after subtracting this, the whole error remained concentrated in the smaller quantity, which represented the heat capacity. In some cases, again, the heat capacity for a given interval had to be found by taking the difference between two different determinations, and in such cases the error was greater.

Results Obtained.

The experimental results are collected in Tables I to X, pp. 23—32. In these w is the weight of substance taken, r the rise or fall measured in the calorimeter, t and t' the initial and final temperatures of

the substance in degrees centigrade (the latter being identical with that of the calorimeter); "Cal." represents the total calories evolved or absorbed, and "Cal. per 1 gram" those evolved or absorbed per unit weight of substance, a deduction having been made for that portion attributable to the bottle and its thermometer, namely $2.223 \times t - t'$; C or *c* is the heat capacity deduced, the range of temperature and mean temperature to which it applies being given under τ , measured in degrees above or below the melting point of the substance in question. Such results as apply to ranges of temperature partially embraced in other determinations are enclosed in square brackets, and are used for deducing those values opposite to which no experimental data appear. The two values given for the heat capacity at 0° (the freezing point of the substance) are those which would be deduced from the determinations at the two higher and two lower temperatures respectively. Where the determinations include the heat of fusion the differences between the individual experiments (not the means) are used in calculating the heat capacity; those determinations are divided into two series, A and B (see Table II), each of which is differentiated separately (Table II continued), and the means of these two series of values taken; then the experiments A and B are taken together alternately, and the two other series thus obtained give another series of mean results, the mean of these two means being finally taken. The heat of fusion was determined from those experiments in which the initial temperature was nearest to the temperature of fusion.

The general results are collected in Table A, where those in the first five lines give the values for 1 gram of substance, and those in the second five the values for a gram-molecular proportion of it.

Details.

Before discussing the general results, the following details may be noticed:—

Sulphuric Acid.—The value found for the liquid at $19^\circ.53$ seems to be rather too high, and this makes the value deduced for $26^\circ.74$ too high, and that for $41^\circ.25$ too low. The value for 0° has here been deduced diagrammatically, the probable error (Table A) being determined from the errors of the duplicate determinations. Both the values for the solid at the initial temperature of $-16^\circ.8$ appeared somewhat anomalous, and they were consequently omitted in the calculations. In taking the mean value for $\tau = 0^\circ$, a double weight has been assigned to the value deduced from the determinations at the two higher temperatures. The probable error (given in Table A) in this, and most other cases, has been deduced in the ordinary way from those two values. The heat capacities of neither the solid nor the liquid show any signs of an abnormal increase as we approach the melting point.

Table A.—Heat Capacity and Heat of Fusion.

Substance.	Liquid.		Solid.		Heat of fusion = l .	C - c.	F. p.	$\frac{l}{C - c}$.	$\frac{l}{t - C - c}$.
	Heat capacity = C.	Probable error.	Heat capacity = c.	Probable error.					
Sulphuric acid.....	0.3355 + 0.00046 <i>t</i>	± 0.0018	0.2721 + 0.00124 <i>t</i>	± 0.0079	24.031 ± 0.160	0.0634	10°-352 C.	379° ± 47°	-308°-0 C.
Monhydr. of sulph. acid. ...	0.4430 + 0.00033 <i>t</i>	± 0.0008	0.2273 + 0.00073 <i>t</i>	± 0.0012	39.918 ± 0.249	0.2157	8.63	185 ± 2	-170.5
Tetrahydr. of calc. nitrate.	0.5185 - 0.00040 <i>t</i>	± 0.0007	0.3973 + 0.00175	± 0.0049	33.402 ± 0.119	0.1212	42.4	270 ± 9	-234.0
Naphthalene	0.4824	± 0.0070	0.3612 + 0.00076 <i>t</i>	± 0.0018	35.625 ± 0.296	0.1212	79.66	294 ± 50	-214.0
Benzene	0.3957 + 0.00109 <i>t</i>	± 0.0070	0.4600 + 0.0049 <i>t</i>	± 0.0313	29.433 ± 0.075	...	5.41		
H ₂ SO ₄ = 97.82	32.82 + 0.045 <i>t</i>	± 0.18	26.62 + 0.121 <i>t</i>	± 0.77	2350.71 ± 15.65	0.20			
H ₂ SO ₄ .H ₂ O = 115.78	61.29 + 0.044 <i>t</i>	± 0.09	26.32 + 0.095 <i>t</i>	± 0.14	4621.71 ± 28.83	24.97			
Ca(NO ₃) ₂ .4H ₂ O = 235.53 ...	122.12 - 0.004 <i>t</i>	± 0.17	93.68 + 0.412 <i>t</i>	± 1.15	7888.61 ± 28.03	28.10			
C ₁₀ H ₈ = 127.7	61.60	± 3.45	46.13 + 0.007 <i>t</i>	± 0.23	4549.3 ± 37.80	15.55			
C ₈ H ₆ = 77.76	30.76 + 0.054 <i>t</i>	± 0.54	35.77 + 0.031 <i>t</i>	± 2.43	2288.7 ± 5.83	...			

Monohydrate of Sulphuric Acid.—The increase in the heat capacities, both of the liquid and the solid, appear to be slightly greater near the melting point than at more distant temperatures, but the differences are within the limits of the errors of the respective determinations. They are:—

Increase.

Solid.....	0·00090 for 1° from	−16°	to	− 7°	(0° = f.p.)
	0·00050	„	−30°	„	−16° „
Liquid ..	0·00045	„	+ 1°·5	„	+18°·3 „
	0·00031	„	+18°·3	„	+29°·2 „

Tetrahydrate of Calcium Nitrate.—The determinations of the heat capacity of the liquid show that there is a decrease instead of the usual increase as the temperature rises. The values are:—

Decrease.

0·00054 per 1° from	−12°·7	to	+ 8°·6	(0° = f.p.)
0·00027	„	+ 8°·6	„	+28°·8 „

One of the determinations, it will be noticed, applied to temperatures entirely below the freezing point. The heat of fusion was determined by placing the bottle with the superfused liquid in the calorimeter till the temperature of the latter had been attained, raising the stopper of the bottle, and inserting a minute crystal of the solid salt; crystallisation then took place at the temperature of the calorimeter, 25° below the normal freezing point, and its value at this latter was calculated by adding to the observed value 25(C−c) cal., C−c being 0·1481 at an average temperature of −12°·5. This method, where practicable, is more accurate than that usually adopted. In the case of the solid salt the rate of increase of the heat capacity is rather greater near the melting point, but the difference is scarcely greater than the experimental error, and would not affect the results to any appreciable extent.

Naphthalene.—The determinations with the liquid applied to one interval of temperature only. The probable error is calculated from the variation of the mean values deduced from the determinations marked A, B, and C, these mean values being the result of combining each of the determinations with those marked α , β , and γ . In the case of the solid the determinations extended over two intervals of temperature, the probable error in the value at 0° being calculated from the difference between the various duplicates. This substance was less fully examined than the others, owing to its having already been investigated by Alluard.

Benzene.—In the case of the liquid, the increase at the higher temperature is rather greater than at the lower one. The values are:—

0.00065 per 1° from 7°·3 to 23°·4 (0° = f.p.)

0.00153 ,, 23°·4 ,, 45°·4 ,,

the average increase being large in comparison with the other substances examined. The values for the solid are somewhat irregular, but, on plotting them out, they are found to be evenly distributed above and below a straight line; the value at 0° and the rate of increase have been deduced from this line, and the probable error was calculated by noting the difference which would be caused by taking the values above or below it.

Comparison with the Results of other Observers.

The results here obtained are compared in Table B with those given by other observers. Except where it is stated otherwise, they refer to the temperatures at which the substances melt under normal conditions. The concordance exhibited by this table is in most cases fairly good, though Berthelot's values for the heat of fusion of sulphuric acid and its monohydrate seem wholly inexplicable.

My value for the heat capacity of liquid calcium nitrate receives confirmation from some determinations which I made some time ago at 18°, with various solutions of salt up to a strength of 61.4 per cent. $\text{Ca}(\text{NO}_3)_2$ by an electrical method; the results obtained formed a fairly uniform curve, and, on extending this up to 69.5 per cent. (the composition of the tetrahydrate), I got 0.517 as the heat capacity of such a solution, while the present results give 0.5185 at 42°·4, or 0.5283 at 18°.

General Results.

From Table A (p. 18) it will be seen that the heat capacity of solid benzene is greater than that of the liquid, so that $C - c$ becomes a negative quantity. This is somewhat remarkable, for in other known instances, such as that of beeswax, where fusion is a gradual process, and the heat capacity of the solid becomes abnormally great as the melting point is reached, this abnormality makes itself evident in the augmenting rate at which the increase occurs, whereas with benzene no such abnormal increase is noticed, although the determinations extend as far as 36° below the melting point: the only sign of anything unusual is that the rate of increase is considerably greater than that in the other cases investigated. This observation with benzene must throw some doubt on conclusions drawn from the heat capacity of any solids, unless the determinations extend through a very long range of temperature. It must be noted that the heat of fusion given here for benzene will be too small if part of that heat of fusion appears as the heat capacity of the solid.

The values for the temperature of no crystallisation obtained from the present results are given in Table C: the only other substances

Table B.—Comparison of Values from various sources.

Substance.	Heat capacity.		Heat of fusion.
	Liquid.	Solid.	
Sulphuric acid	0·3355 + 0·00046 <i>t</i> P.	—	24·031 P.
	0·3297 + 0·00024 <i>t</i> M.	—	8·78 B.
	0·3053 + 0·00106 <i>t</i> Pf.	—	—
	0·3415 + 0·00033 <i>t</i> Pf.	—	—
	0·3095 at 24°·7 Per.	—	—
	0·3421 „ P.	—	—
	0·3430 at 33°·5 K.	—	—
	0·3461 „ P.	—	—
Monohydrate of sulphuric acid	0·4430 + 0·00038 <i>t</i> P.	—	39·918 P.
	0·4300 + 0·00014 <i>t</i> Per.	—	31·7 B.
	0·4170 + 0·00071 <i>t</i> Pf.	—	—
Naphthalene	0·4824 at 83° to 98° P.	0·3612 + 0·00076 <i>t</i> P.	35·625 P.
	0·4186 at 88° to 99° A.	{ 0·3315* + 0·00015 <i>t</i> } A. { 0·3642† + 0·00062 <i>t</i> }	35·679 A.
Benzene....	0·3957 + 0·00109 <i>t</i> P.	—	29·433 P.
	0·3860 + 0·00160 <i>t</i> S.	—	29·089 Pet.†
	0·4164 at 25° P.	—	—
	0·4158 „ S.	—	—
	0·4380 at 45° P.	—	—
	0·436 „ R.	—	—

P. = Pickering.

M. = Marignac ('Lieb. Ann.,' Suppl. 8, p. 355).

Pf. = Pfaundler ('Jl. Prakt. Chem.,' vol. 101, p. 508; and 'Berlin Berichte,' 1870, p. 798).

Per. = Person ('Ann. Chim. Phys.' (3), vol. 33, p. 446).

K. = Kopp ('Pogg. Ann.,' vol. 75, p. 98).

B. = Berthelot ('Compt. Rend.,' vol. 78, p. 716).

A. = Alluard ('Ann. Chim. Phys.' (3), vol. 57, p. 438).

S. = Schüller ('Pogg. Ann.,' Ergänzt. 5, p. 125).

Pet. = Pettersson ('Jl. Prakt. Chem.,' vol. 24, p. 129).

R. = Regnault ('Mém. de l'Acad.,' vol. 26, p. 262.)

* This is deduced from Alluard's values for ranges from 0°·5 to 19°, and 20° to 65°.

† From Alluard's values for ranges from -26° to +7°·7 and 0°·5 to 19°; to the former of these, however, he attached less value than to those for higher temperatures.

‡ Pettersson's value was obtained with benzene freezing at 4°·97.

for which the data are sufficient to calculate the values for C and c at the fusing point are given there also; these are, water, naphthalene, using Alluand's values, and bromine. Regnault's determinations of the heat capacity of ice at low temperatures ('Ann. Chim. Phys.' [3], vol. 26, p. 286) have been combined with Person's, and give 0.4757 for its value at 0° ; this brings the temperature of no crystallisation to -167° , a value differing but little from Person's, -160° . Regnault's determinations with bromine ('Ann. Chim. Phys.' (3), vol 9, p. 344) give a result of doubtful value. He gives the fusing point as -7.32 , the heat of fusion as 16.185 , and sufficient data to calculate the heat capacity of the liquid as $0.1005 + 0.00028t$, but for that of the solid his data are insufficient; they may be taken as indicating, though very doubtfully, $0.1038 + 0.00047t$, a value which gives $C-c$ negative; or, if we take the mean of them, we get 0.0843 as the heat capacity at -48.96°C. , a value which gives -992° for the temperature of no crystallisation: as the heat capacity at the temperature of fusion must be greater than at -48.96 , we may safely say that a temperature lower than -992° is indicated as that of no crystallisation. Some determinations with pentahydrated sodium thiosulphate were made by Trentinaglia ('Wien. Akad. Ber.,' vol. 72, Abth. II, p. 669), with the express object of testing the validity of Person's conclusion, but as the values for the heat capacities were determined at one temperature only, and that not very close to the melting point, I do not think that any conclusions can be drawn from them. The probable error of my own results was calculated by taking half that which would be caused by taking such of the extreme values for C , c , and l given in Tables I to X as would affect the result in the same direction.

Table C.

Substance.	$t - \frac{f}{C-c}$.
Water.....	-160°C. (Person, &c.
Monohydrate of sulphuric acid	$-177^\circ \pm 2$ (Pickering)
Naphthalene	$-214^\circ \pm 50$ „
„	-333° * (Alluand)
Tetrahydrate of calcium nitrate	$-234^\circ \pm 9$ (Pickering)
Sulphuric acid	$-369^\circ \pm 47$ „
Bromine	$> -992^\circ$ (Regnault)

These being the only results available for testing Person's view, that the temperature of no crystallisation is -160° for all substances, we must certainly conclude that this view has not yet been established.

* Taking the other value for c deducible from Alluand's results (0.3642), we get -583° .

Table I.—Liquid Sulphuric Acid. Freezing point = $10^{\circ}\cdot352$ C.

w.	r.	t° C.	t° C.	Cal.	Cal. per gram.	C.	τ (f.p. = 0°).
—	—	—	—	—	—	0·3355	0
57·37	0·3501	7·237	17·391	221·19*	3·462	0·3409	(-3·62 to +7·04 =) 1·81
"	0·3738	6·224	17·384	236·20*	3·685	0·3302	
53·55	0·4388	32·214	18·459	277·27*	4·607	0·3349	(22·54 to 8·15 =) 15·35
"	0·4902	33·559	18·534	309·73*	5·160	0·3434	
53·31	0·7478	41·255	18·599	472·47*	7·919	0·3495	(30·94 to 8·11 =) 19·53
"	0·7421	41·329	18·650	468·87*	7·850	0·3478	
58·95	1·5805	62·090	19·180	982·68†	15·052	0·3508	(51·49 to 8·66 =) 30·08
"	1·5622	61·601	18·834	971·31†	14·864	0·3476	(22·54 to 30·94 =) 26·74
—	—	—	—	—	—	0·3582	(30·94 to 51·49 =) 41·25
—	—	—	—	—	—	0·3497	

* Water equivalent of calorimeter, &c. = 631·83.

† Water equivalent = 621·75.

Table III.—Liquid Monohydrate of Sulphuric Acid. Freezing Point = $8^{\circ}53$ C.

w.	r.	t° C.	Cal.	Cal. per gram.	C.	τ (f.p. = 0°).
57.61	0.6262				0.4417 } 0.4430	0
"	0.6470	17.187	389.32*	6.2237	0.4442 }	(-5.59 to +8.63 =) 1.52
52.09	0.6099	17.133	402.29*	6.4195	0.4496 }	
"	0.7036	18.485	385.33†	6.7606	0.4397 }	(26.54 to 10.05 =) 18.30
"	0.9129	36.652	444.58†	7.8078	0.4527 }	
"	0.8976	41.667	590.25†	10.351	0.4474 }	(32.58 to 10.14 =) 21.36]
"		40.559	567.13†	9.9537	0.4503 }	
					0.4547 }	(26.54 to 32.58 =) 29.56
					0.4550 }	

* Water equivalent = 621.75 c.c.

† Water equivalent = 631.83.

Table IV.—Solid Monohydrate of Sulphuric Acid.

w.	t°	t° C.	t° C.	Cal.	Cal. per gram.	Cal. absorbed till liquid.	τ (f.p. = 0°).	Heat of fusion.
56.32	1.3887	6.703	17.048	2535.9	44.601*	40.808 A	-1.827	40.387
—	1.3670	5.666	17.162	2496.4	43.865*	39.812 B	-2.864	39.182
55.47	1.3940	2.831	17.109	2545.7	45.322*	41.503	-5.699	40.184
56.32	1.4485	—	2.651	2645.3	46.190*	42.378 A	-11.201	
"	1.4600	—	4.295	2666.4	46.488*	42.666 B	-12.825	
"	1.5157	—	10.210	2768.1	48.072*	44.267 A	-18.740	
"	1.5257	—	12.766	2786.1	48.295*	44.524 B	-21.296	
57.08	1.7019	—	30.550	3108.0	52.602*	48.879 A	-39.080	
"	1.7187	—	31.087	3138.7	53.120*	49.419 B	-39.617	

* Water equivalent = 1826.2.

c.	r.	c.	r.	Means.			r.
				A and B alone.	A and B together.	General.	
A series alone.							
0.1675	(- 1.8 to - 11.2 =)	0.2835	(- 2.9 to - 12.8 =)			0.2301 } 0.2273 0.2245 }	0
0.2506	(- 11.2 to - 18.7 =)	0.2217	(- 12.8 to - 21.3 =)				
0.2267	(- 18.7 to - 39.1 =)	0.2672	(- 21.3 to - 39.6 =)				
B series alone.							
A and B series together.							
0.1689	(- 1.8 to - 12.8 =)	0.3042	(- 2.9 to - 11.2 =)		0.2252	0.2366	- 7.18
0.2707	(- 12.8 to - 18.7 =)	0.2126	(- 11.2 to - 21.3 =)		0.2362	0.2417	- 16.01
0.2407	(- 18.7 to - 39.6 =)	0.2446	(- 21.3 to - 39.1 =)		0.2470	0.2459	- 29.68

Table V.—Liquid Tetrahyd rate of Calcium Nitrate. Freezing point $42^{\circ} \cdot 4$ C.

<i>w.</i>	<i>t.</i>	t° C.	t° C.	Cal.	Cal. per gram.	C.	τ (f.p. = 0°).
—	—	—	—	—	—	0·5196 } 0·5185 0·5175 }	0
50·51	—	39·824	18·506	614·47*	11·227	0·5266 } 0·5271 }	(-1·53 to -23·85) - 12·69
"	—	41·928	18·583	673·42*	12·305	0·5228 } 0·5164 }	(18·63 to -23·97) - 2·67
52·36	—	60·279	18·416	1238·9*	21·884	0·5180 } 0·5162 }	(38·94 to -22·58) + 8·18
"	—	61·770	18·444	1265·0*	22·322	0·5154 0·5099	(-1·53 to 18·63) + 8·55
55·36	—	80·993	19·243	1908·1†	31·987		(18·63 to 38·94) + 28·79
"	—	81·690	20·387	1888·1†	31·644		
—	—	—	—	—	—		

* Water equivalent = $621 \cdot 75$. † Water equivalent = $631 \cdot 83$.

Table V (continued).—Heat of Solidification.

<i>w.</i>	<i>t.</i>	t° C.	Cal.	Cal. per gram.	Cal. for raising solid from t to t' .	Heat of fusion.	τ (f.p. = 0°).
50·51	—	—	—	—	—	—	—
"	—	—	—	—	—	—	—
52·36	—	—	—	—	—	—	—
"	—	—	—	—	—	—	—
55·36	—	—	—	—	—	—	—
"	—	—	—	—	—	—	—
—	—	—	—	—	—	—	—

* Water equivalent = $1826 \cdot 2$.

Table VI.—Solid Tetrahydrate of Calcium Nitrate.

m.	r.	t° C.	t° C.	Cal. total.	Cal. per gram.	C.	τ (f.p. = 0°).
—	—	—	—	—	—	0.4047 0.3899	0°
50.51	0.7892	41.660	18.334	498.61*	8.845	0.3973	(- 0.80 to -23.8 =) -12.3
"	0.7711	41.032	18.334	487.20*	8.646	0.3801	
55.12	0.5636	0.222	17.482	356.10*	5.764	0.3809	(-25.0 to -42.2 =) -33.61
"	0.5549	0.198	17.272	358.75*	5.820	0.3340	
54.59	1.3758	-26.937	16.805	855.42†	13.889	0.3409	(-25.6 to -70.8 =) -48.20]
"	1.4862	-29.873	16.825	924.02†	15.829	0.3175	
—	—	—	—	—	—	0.3218	(-42.2 to -70.8 =) -56.50
						0.3019	

* Water equivalent = 631.83.

† Water equivalent = 621.75.

Table VII.—Liquid Naphthalene. Freezing point = $79^{\circ}\cdot866\text{ C}$.

<i>w</i> .	<i>r</i> .	$t^{\circ}\text{C}$.	$t^{\circ}\text{C}$.	Cal.	Cal. per 1 gram.	Cal., less that given out by solid.	τ (f.p. = 0°).	Heat of fusion.
29.23	1.0091	81.692	18.161	1842.8*	58.154	37.310	+ 1.806 <i>a</i>	$\left. \begin{array}{l} 36.439 \\ 34.909 \\ 35.536 \end{array} \right\} 35.625$
"	0.9992	84.035	18.977	1824.7*	57.477	36.910	+ 4.149 <i>b</i>	
"	1.0112	84.428	19.106	1846.6*	58.207	37.682	+ 4.542 <i>γ</i>	
"	1.1130	97.375	19.134	2032.6*	63.589	43.073	+ 17.489 A	
"	1.1392	98.100	18.396	2080.4*	65.044	44.279	+ 18.214 B	
"	1.1645	98.769	18.359	2126.6*	66.560	45.791	+ 18.883 C	

A.		B.		C.		Mean.
C.	τ .	C.	τ .	C.	τ .	
<i>a</i> 0.3675	9.65	0.4247	10.01	0.4966	10.19	$\left. \begin{array}{l} \\ \\ \\ \end{array} \right\} 0.4824$ (21.66 to 0 =) 10.83
<i>β</i> 0.4620	10.83	0.5239	11.18	0.6028	11.51	
<i>γ</i> 0.4164	11.03	0.4825	11.38	0.5654	11.71	
Mean. 0.4153	10.50	0.4770	10.86	0.5549	11.14	

* Water equivalent = 1826.2.

Table VIII.—Solid Naphthalene.

w.	r.	t° C.	t ^m C.	Cal.	Cal. per gram.	C.	τ (f.p. = 0°).
30·69	1·1075	73·764	18·062	699·77*	18·767	0·3369 } [0·3381	(- 6·72 to 61·77) - 34·24]
"	1·0874	72·565	18·180	687·04*	18·447	0·3392 }	
30·25	0·4350	40·964	17·777	274·84*	7·382	0·3183 }	(- 39·49 to 62·13) - 50·81
"	0·4235	39·824	17·731	267·56*	7·221	0·3269 }	(- 6·72 to 39·49) - 23·11
						0·3436	0
						0·3612	

* Water equivalent = 631·83.

Table IX.—Liquid Benzene. Freezing point 5°41.

w	r	t° C.	t ^m C.	Cal.	Cal. per gram.	C.	τ (f.p. = 0°).
—	—	—	—	—	—	0·3850 } 0·3957	0
28·48	0·3016	6·501	19·504	187·53*	5·534	0·4059 }	(1·22 to 14·12 =) 7·75.
"	0·2857	6·772	19·567	177·60*	5·237	0·4114 }	
27·22	0·4842	40·312	17·798	305·91†	9·399	0·4093 }	(35·20 to 12·38 =) 23·79.
"	0·4950	40·515	17·797	312·75†	9·634	0·4175 }	
27·55	0·9992	62·208	18·504	621·25*	19·024	0·4241 }	(56·49 to 13·05 =) 34·77].
27·51	0·9878	61·604	18·428	614·15*	18·833	0·4253 }	
—	—	—	—	—	—	0·4358 }	(35·20 to 56·49 =) 45·90.
						0·4545	

* Water equivalent = 621·75. † Water equivalent = 631·83.

Table X.—Solid Benzene.

<i>w.</i>	<i>t.</i>	<i>t</i> ° C.	<i>t</i> ° C.	Cal.	Cal. per 1 gram.	Cal. per 1 grain solid.	<i>τ</i> (f.p. = 0°).	Cal. from <i>τ</i> to 0.	Heat of fusion.
28.48	1.6289	4.006	16.605	1012.7*	34.567	30.069 B	- 1.404	0.646	29.423
"	1.6363	3.927	16.409	1017.4*	34.747	30.328 A	- 1.483	0.683	29.645
27.78	1.6434	0.618	17.079	1038.4†	36.062	31.369 B	- 4.792		
"	1.6611	0.442	16.582	1052.0†	36.577	32.087 A	- 4.968		
29.82	1.9711	- 6.162	16.538	1225.5*	39.403	34.931 A	- 11.572		
"	1.9523	- 6.818	16.305	1213.8*	38.981	34.604 B	- 12.228		
28.25	1.9592	- 12.800	16.442	1237.9†	41.523	37.091 B	- 18.214		
"	1.9612	- 13.568	16.568	1239.2†	41.498	37.015 B	- 18.978		
"	2.0184	- 15.802	16.416	1275.3†	42.620	38.198 A	- 21.212		
29.82	2.2815	- 21.178	16.567	1418.5*	44.755	40.272 B	- 26.588		
29.76	2.2816	- 22.391	16.188	1418.6*	44.785	40.455 A	- 27.801		
"	2.3630	- 27.354	16.261	1469.2*	46.092	41.733 B	- 33.020		
29.82	2.4484	- 30.514	16.310	1518.8*	47.441	43.062 A	- 35.924		

* Water equivalent = 621.75.

† Water equivalent = 631.83.

IV. "On Wolf and Rayet's Bright-Line Stars in Cygnus." By
WILLIAM HUGGINS, D.C.L., LL.D., F.R.S., and Mrs. HUGGINS.
Received November 25, 1890.

In 1867 MM. Wolf and Rayet discovered at the Paris Observatory three small stars in Cygnus, which in the spectroscope showed several bright lines upon a continuous spectrum.* All three stars have a very bright band in the blue part of the spectrum.

These stars are :—

B.D. +35°, No. 4001.

B.D. +35°, No. 4013.

B.D. +36°, No. 3956.

Their spectra were described in 1873, by Vogel, whose observations agree substantially with the original description given by Wolf and Rayet.† A more complete account of their spectra was given by Vogel in 1883, from observations at Vienna with the 27-inch refractor made by Sir Howard Grubb.‡

Vogel's measures of the bright blue band place it in the star No. 3956 at from λ 468 to λ 461, with a maximum at λ 464; in the star No. 4013 with a maximum at the same place in the spectrum; while the corresponding blue band in the star No. 4001 has a considerably less refrangible position, commencing at λ 470, reaching a maximum at λ 468, and ending about λ 465.

These later measures, though they differ from his earlier ones, in so far as they show that the blue band has not an identical position in all three stars, nevertheless support substantially his earlier observations, which Vogel considered to show, contrary to the statements of Secchi, that the bright lines, including the blue band, were not due to carbon.

In the diagram, Nos. 1, 2, and 3 show the positions of the bright bands in the three stars, according to Vogel's measures, relatively to the blue band of the hydrocarbon flame.

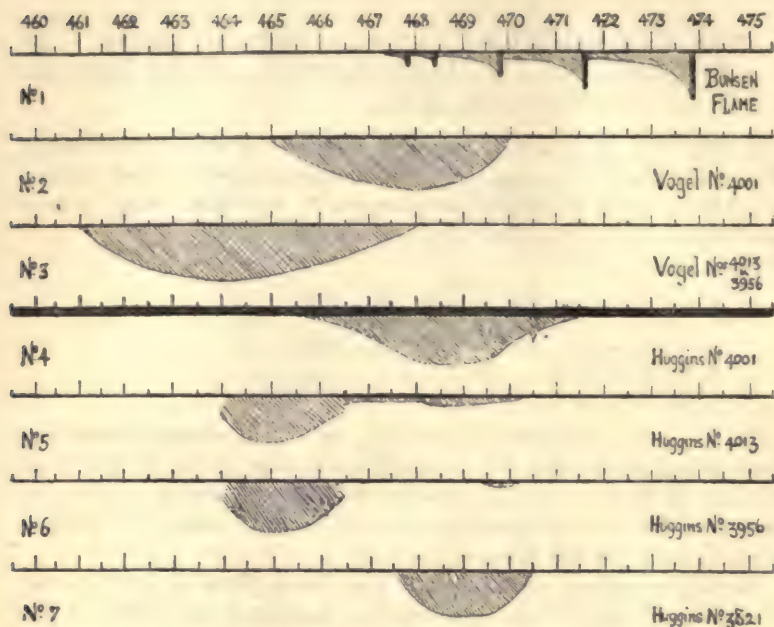
Vogel's measures are :—

	Beginning of the band.	Brightest part.	End of the band.
Star No. 4001.....	λ 470	λ 468	λ 465
„ 4013.....	—	λ 464	—
„ 3956.....	λ 468	λ 464	λ 461

* 'Comptes Rendus,' vol. 65, 1867, p. 292.

† 'Berichte K. Sächs. Ges. der Wiss.,' Dec., 1873, p. 556.

‡ 'Publicationen Astrophys. Observ. Potsdam,' vol. 4, No. 14, pp. 17—21.



His diagram shows the band in No. 4013 to begin and end at about the same positions as in the star 3956.

It has been stated recently that the bright blue band in all three stars is the carbon band in the blue, commencing near λ 474;* and more recently, notwithstanding the difference of position, according to Vogel, of the band in one of the stars from that which it occupies in the other two of as much as λ 0040, that direct comparisons showed

* Professor Lockyer, in the Bakerian Lecture for 1888 ('Roy. Soc. Proc.' vol. 44, p. 37), says of the star No. 4001:—"The bright band with its maximum at λ 468 is the bright carbon fluting commencing at λ 474 and extending towards the blue, with its maximum at 468, as photographed at Kensington."

Of the star 4013:—"The bright band in the blue at 473 is most probably the carbon band bright upon a faint continuous spectrum, this producing the absorption from 486 to 473" (*loc. cit.*, p. 41).

Of the star No. 3956:—"The bright band at 470 is the carbon band in the blue, commencing at 474, with its maximum at about 468, as observed and photographed at Kensington" (*loc. cit.*, p. 43). See Vogel's measures for the band in this star, which are given in the text.

Diagrams of the spectra of these stars are given at pp. 38, 40, and 41, based on Vogel's observations and his curves, which, on a slightly reduced scale, are placed at the bottom of the diagrams. The maximum of Vogel's curves is placed in all three diagrams at λ 468, and agrees in the diagrams with the carbon band, whereas Vogel's original curves and his measures place the maximum in the case of two of the stars at λ 464, beyond the carbon band.

an absolute coincidence of the band in all three stars with the blue band of a spirit-lamp flame.*

As the presence or absence of carbon in these stars, as shown by the coincidence or otherwise of the blue band with that of the hydrocarbon flame, was of great importance to us in connexion with a wider investigation on which we are at work, we thought it necessary, after these recent statements as to the position of the band, to make direct comparisons of the spectra of these stars with that of the hydrocarbon flame under sufficiently large dispersion to enable us to determine whether Vogel's measures are substantially correct, or whether they are so largely in error as the absolute coincidence of the band with the blue band of a spirit-lamp flame in the case of all three stars would show them to be.

The obvious importance of making the observations with sufficient dispersion is supported by Vogel's own experience. With the small dispersion which he employed in his earlier observations in 1873, he did not detect the large difference of position, about λ 0040, of the band in No. 4001, as compared with its position in the other two stars. On this point Vogel says, in his memoir of 1883:—"Etwas abweichend ist nur die Auffassung der Lage der breiten hellen Bande im Blau, die bei den früheren Messungen bei allen drei Sternen übereinstimmt. . . . Bei den verhältnissmässig geringen optischen Hilfsmitteln, mit denen jene Messungen ausgeführt wurden, ist die Uebereinstimmung aber eine ganz überraschende" (*loc. cit.*, p. 21).

We observed the spectra of the stars successively, first with a direct vision prism of small dispersion, then with a spectroscope (A) containing one prism of 60° , and finally with a spectroscope (B) with two compound prisms, equal to about four prisms of 60° ; with the last-named instrument the comparisons with the hydrocarbon flame were made.

A rapid preliminary comparison in the spectroscope (B) of the spectra of the three stars with the blue base of a Bunsen flame

* Professor Lockyer, in a signed article in 'Nature' (August 7, 1890, vol. 42, p. 344), writes;—

"In the Bakerian Lecture for 1888 I gave a complete discussion of the spectra of bright-lined stars, as far as the observations went, and the conclusion arrived at was that they were nothing more than swarms of meteorites a little more condensed than those which we know as nebulae. The main argument in favour of this conclusion was the presence of the bright fluting of carbon which extends from 468 to 474. This standing out bright beyond their short continuous spectrum gives rise to an apparent absorption band in the blue. . . . Direct comparisons of the spectrum of all the three stars in Cygnus with the flame of a spirit-lamp have been made by Mr. Fowler, and these showed an absolute coincidence of the bright band in the stars with the blue band of carbon seen in the flame. It was found quite easy to get the narrow spectrum of the star superposed upon the broader spectrum of the flame so that both could be observed simultaneously."

showed at once the substantial accuracy of Vogel's measures, and the striking difference of position of the band in the star No. 4001 from that which it holds in the other two stars.

The obvious want of agreement of the star bands with the blue band of the Bunsen flame was seen at once. Their relative positions appeared to agree substantially with the positions represented in No. 2 and No. 3 of the diagram, which are based on Vogel's measures. More careful and repeated observations brought out clearly, as is indeed shown by Vogel's curve, that the star bands differ in character as well as in position from the blue band of the hydrocarbon flame, and also in some respects from each other.

Before giving in more detail the results of our observation on each of the three stars, it should be stated that in all the stars the continuous spectrum is not in our instruments a short one, ending before the position of the bright blue band is reached. On the contrary, an examination with all three spectroscopes showed that the continuous spectrum, though enfeebled by absorption a little before reaching the blue band, can be traced, as is shown in Vogel's curves, quite up to the band, and indeed extends for a long distance into the violet beyond the blue band. The blue band does not in our instruments stand out bright beyond the end of a short continuous spectrum, but falls upon a fairly luminous continuous spectrum, which can be traced past the blue band into the violet, apparently as far as the eye could be expected to follow it.

We suspected bright lines or bands in the region more refrangible than the blue band, but in such faint objects this is a point which should be determined by photography.

Professor E. C. Pickering has since kindly informed us that his photographs of the star No. 4001, which extend into the ultra-violet region, show beyond the blue band the bright hydrogen lines at 434, 410, 397, and 389; and also other bright lines at 462, 455, 420, 406, 402, 395, and 388.

In his photographs of the stars 4013 and 3956, however, the only well-marked line is in the blue at 470.

Star 4001.—In this star, as is shown by Vogel's measures and curve, the bright blue band is less refrangible than in the other two stars, and approaches therefore nearer to the position of the blue band of the hydrocarbon flame. The appearance and position of the band in the star as contrasted with that of carbon, when observed in spectroscope B, are represented in spectrum No. 4 of the diagram.

The brightest part of the band, from about λ 468 to λ 469, falls off rather suddenly in brightness at about these wave-lengths, but can be traced towards the red as far as about λ 471.5, and as far in the blue as about λ 465.5.

In our observations of this and the other stars we did not attempt micrometric measures of the blue band, but we estimated their positions by means of the intervals between the five flutings of the band of the Bunsen flame. In the case of objects so faint in our instrument when viewed under the dispersion of spectroscope B, we did not consider there would be any real gain of accuracy by attempting to take measures.

Though the wave-lengths assigned to our positions must therefore be regarded as not more than approximately correct, we have no hesitation in considering them fully accurate enough for the purpose of our investigation.

The star band is not split up into well-separated maxima, as is the Bunsen flame band, but we have little doubt that the brightest part of the band, from $\lambda 468$ to $\lambda 469$, which is much, and rather suddenly, brighter than its beginning and termination, consists of bright lines. Lines appear to flash out at moments, but in our instruments they cannot be seen with sufficient steadiness for us to be sure of their number and position.

Under certain conditions of the electric discharge, the normal relative brightness of the component flutings of the blue hydrocarbon band has been observed to be so far changed that the position of maximum intensity is moved from the less refrangible end of the band towards the blue end; but the five flutings remain without any change of their position in the spectrum.*

Dr. Hasselberg, by means of feeble disruptive discharges from tin-foil terminals placed outside an exhausted tube containing vapour of benzole, obtained a nearly pure spectrum of the order of that in a hydrocarbon flame mixed only with faint lines of hydrogen. He says: "Es war aber hier die violette Gruppe sehr schwach. Dagegen schien mir die blaue Gruppe relativ heller als im Flammenspectrum, und sie hatte ausserdem entschieden ihre grösste Intensität nicht an der weniger brechbaren Kante, sondern mehr nach dem Violetten hin. Dasselbe schien mir auch mit der gelben Gruppe der Fall zu sein. In Bezug auf die grüne Gruppe konnte ich aber keine Verschiebung des Intensitätsmaximums bemerken."

Dr. Hasselberg gives curves to show the amount of this change of intensity in the blue group and in the orange group. In the blue group the maximum is moved from the first to the third line, that

* "It is necessary to state that the maximum luminosity of the blue band, under some conditions, is about 468. . . . The conditions under which this band has its maximum luminosity at 468 in Geissler tubes seem to be those of maximum conductivity. If the pressure be high, all the members of the group are sharp, and the luminosity of the band is almost uniform throughout. This always occurs when the pressure is very low. At intermediate stages of pressure, however, the luminosity has a very decided maximum at about 468" (Appendix to the Bakerian Lecture for 1888, 'Roy. Soc. Proc.,' vol. 45, pp. 167, 168).

is, to about λ 4698. His curve gives the brightness of the maximum over that of the first line as about 7 to 6, whereas the normal relative intensity of these two lines is in the inverse direction and as about 2 to 4 (Watts, 'Index of Spectra,' p. 30).*

A similar change from the normal relation of brightness of the flutings within the band, even if removed to λ 468, does not seem to us to bring the star band sufficiently into accordance in character and position with those of the band of the hydrocarbon flame to justify us in attributing the blue band in the star to carbon. Though we traced the band a little further towards the red, than the position of the beginning of the band given by Vogel's measures, yet it is very faint, and without any increase in brightness at the place of the second fluting of the carbon band, beyond which we were unable to see it.

According to Hasselberg's curve, the second bright fluting, where in our instruments the star band ends, still retains a brightness of about 11/12 of that of the maximum, and the first line, at the position of which no brightening of the feeble continuous spectrum of the star could be detected, a brightness of about 6/7 of that of the maximum. That the flutings of the band were not obscured by the absorption band at this part of the spectrum appears clear from the circumstance that we could trace the faint continuous spectrum up to the bright band.

Vogel's and our observations agree in making the band run on some distance beyond the visible termination of the blue band of the Bunsen flame. Piazzì Smyth, under some conditions, observed a large number of faint "linelets" beyond the "5th leader" of the band, where its visibility usually ends; and in the brilliant light of the arc the band can be traced further in the blue. The extension of the band under such circumstances does not seem to us to affect our present argument; for in the very feeble light of the star we may surely take it that the carbon band, if present, could not be seen to extend further than its usual visible limit in a Bunsen flame, namely, about λ 468.

Perhaps it should be stated in connexion with the circumstance that we saw the band extend a little further towards the red than Vogel did, that at the time of our observations the hydrogen line at F was not visible in our instruments, whereas it was bright at the time when Vogel observed the star. In the spectrum of a similar star, D.M. +37° 3821, in which the hydrogen line at F at the time was bright, the blue band was seen by us to stop near the place given by Vogel in his measures of the star No. 4001.

Not only is there no coincidence, so far as Vogel and we have observed, of the position of the band in the star with that of the blue

* 'Mém. de l'Acad. Imp. des Sciences de St. Pétersbourg,' vol. 22, No. 2, 1880, p. 82.

band of the Bunsen flame ; but, further, the want of accordance of its general characters is so great as to make the view that its origin is carbon very improbable. This improbability is very greatly increased when we find, as will be shown presently, that no traces whatever of the very bright beginnings of the more brilliant green and orange bands could be detected by us in any of the stars. Further, Professor E. C. Pickering has kindly sent to us an account of his photographs of this star, which, though they show the hydrogen line at λ 434, do not exhibit any brightness at the positions of the indigo hydrocarbon bands, beginning near 4312, and λ 4382.

This star, however, can scarcely be taken by itself ; in the case of the other two stars, in the spectra of which, according to Vogel's, Copeland's, and our own observations, the brightest part of the blue band is from λ 464 to λ 465, but nearer λ 465, quite outside the ordinary visible limit of the carbon band, the evidence seems very strong indeed that the band does not owe its origin to carbon.

We satisfied ourselves that when the spectrum of the star is examined under the dispersion of spectroscope B, none of the brighter parts of its spectrum fell at, or very near, the green, orange, and indigo flutings of the hydrocarbon flame spectrum ; at these positions we were unable to detect any sensible brightening of the star's spectrum. Professor Copeland's measure of the blue band in 1884 was λ 469.5.

No. 4013.—Vogel does not give measures of the beginning and the ending of the band in this star, but only of the brightest part :—“ Hellste Stelle, nahezu Mitte, einer breiten verwaschenen Bande, λ 464.” He gives, however, a diagram of the spectrum in which the bright blue band is represented as substantially coincident in position and in general character with that in the spectrum of No. 3956.

Our observations agree substantially with those of Vogel, but they make the band to consist of two parts—a very bright part, from about λ 466 to λ 464, but brightest near λ 465, and a very faint band, apparently detached from the bright one from about λ 4685 to about λ 4705. This faint band is brightest near where it ends rather abruptly at the more refrangible end. The very bright band has not the character of a fluting, nor is it broken up into maxima widely separated like those of the Bunsen flame band, but appears to be a group of bright lines. The lines were only glimpsed at moments ; it is therefore difficult to make a drawing which truly represents the character of the band as seen in our instruments. The band, which is shown at No. 5 of the diagram, is left unfinished at the more refrangible end, as we were not certain how far we ought to consider it to extend.

In this star (as we shall show to be the case in No. 3956 also), the

great body of bright radiation lies far beyond the ordinary visible limit of the blue carbon band, and no connexion whatever with carbon is even suggested to us by the star's spectrum. Dr. Copeland's measure of the band in 1884 was λ 465.4.

The continuous spectrum of the star is unequally bright from the presence of bright groups and also apparently of absorption bands or lines, and therefore with small dispersion it might be easily supposed that the spectrum is brighter at the position of the green carbon band. We examined the continuous spectrum repeatedly with great care, and we were able to satisfy ourselves that, under the considerable dispersion of our instruments, there was no sensible brightening of the spectrum at the positions of the green and of the orange bands of the Bunsen flame.

No. 3956.—Vogel places the brightest part of the band in this star at the same position in the spectrum as in the star last considered. No. 4013, namely, at λ 464, a position beyond the carbon band. The position of the band as it appeared in spectroscope B with the third eye-piece, is represented at No. 6 in the diagram. The position of the band relatively to that of the Bunsen flame was determined by estimations made by means of the intervals between the bright flutings of the Bunsen band. The position agrees substantially with that given by Vogel, but places the maximum brightness nearer to 465. This bright part probably consists of a group of bright lines and falls off rather suddenly at both ends. We were not certain if the light beyond this bright part was due to a continuation of the band or to the continuous spectrum, more or less dimmed by absorption; we have, therefore, left the ends of the band incompleting in the diagram. Copeland's measure of this band in 1884 was λ 464.9.

The sub-band seen in the star No. 4013 is very much fainter in this star, but we have little doubt that there is a very faint band present at about the same place in the spectrum.

Professor E. C. Pickering has found in the near neighbourhood of these three stars other stars possessing bright lines in their spectra.* The brightest of these, independently discovered by Dr. Copeland in 1884,† namely, D.M. +37° 3821, in which the spectrum is similar to that of the Wolf-Rayet stars, was examined. Dr. Copeland says of this star:—"It has a spectrum of several bright lines near D, and a very bright band in wave-length 464" (*loc. cit.*). We were therefore

* "The following list contains the designations of all eight stars (with bright lines), the first four being those previously known:—35° 4001, 35° 4013, 36° 3956, 36° 3987, 37° 3821, 38° 4010, 37° 3871, 35° 3952 or 3953. Of these 37° 3871 is P Cygni, and 37° 3821 is the star in the spectrum of which the bright lines are most distinct" (letter in 'Nature,' vol. 34, p. 440).

† 'Monthly Notices, R.A.S.,' vol. 45, p. 91, 1884.

surprised to find the blue band, which is very brilliant, not in the position of the band in the stars No. 4013 and No. 3956, but less refrangible, corresponding to the position of the band in the star No. 4001.

The bright band begins about λ 467 and runs on to nearly λ 470.5. It is clearly not made up of flutings similar to those of the Bunsen flame, but is a group of lines nearly uniformly bright throughout the length of the band. The band did not appear to extend in our instruments towards the red quite so far as the band of No. 4001; it stops near the place assigned by Vogel to the beginning of the band of No. 4001.

The band is represented in spectrum No. 7 in the diagram. Direct comparison with hydrogen showed that the line at F is brilliant in this star.

After some scrutiny of this part of the star's spectrum, we became conscious of a very feeble brightening of the spectrum beyond the bright band towards the violet, and as far as we could estimate its position, at about from λ 464 to λ 467, that is to say, about the position assigned to the band by Dr. Copeland in 1884.

We then re-examined the spectrum of No. 4001, and were able to feel pretty sure that a similar faint brightening of the spectrum occurs in this star also at the same place, namely, about the more refrangible position of the blue band in the stars No. 4013 and No. 3956.

Dr. Copeland, during his travels in the Andes in 1883, observed γ Argûs, and five small stars with bright lines in their spectra. He says:—"As far as my measures and estimates go, all of them belong to the same class as the three Wolf-Rayet stars in the Swan, to which Professor Pickering has since added a fourth outlying member."*

Dr. Copeland gives the position of the bright blue band in γ Argûs as λ 464.6.

Among the stars in the great cluster G.C. 4245, near ζ Scorpii, Dr. Copeland found a star, P. XVI 204 = Stone 9168, which has a similar spectrum, namely, with a bright band in the blue and two in the yellow. He found the position of the blue band to be λ 465.1.

In the case of two other small stars with similar spectra, he found respectively for the blue band the approximate measures λ 463.3 and λ 463.6.

These four stars were similar, therefore, at the time of the observations to No. 4013 and No. 3956, in which the maximum of the blue band is not far from λ 464, and therefore outside and beyond the ordinary visible limit of the blue carbon band.

* "An Account of some recent Astronomical Experiments at High Elevations in the Andes;" 'Copernicus,' vol. 3, 1883.

Professor Vogel observed two other stars with similar spectra, of which the main feature is the very bright band in the blue region, namely, Arg. Oeltzen 17681 and Lal. 13412. These stars are too low in southern declination to be reached from our observatory.

Vogel places the blue band in Lal. 13412 at λ 469, which shows that it has a position similar to that of No. 4001 and of Dr. Copeland's star. In the case of Arg. Oeltzen 17681, Vogel makes the band to extend through about the entire range of refrangibility occupied by the two positions of the blue band in the Wolf-Rayet stars according to his measures of them, namely, from λ 461 to λ 470, with a maximum at the place where they would overlap, namely, λ 466.

Let us consider the four stars with an intensely brilliant blue band which we have examined; in two of them the band extends from about λ 464 to λ 467, and in the other pair the band has a less refrangible position, from about λ 466 to λ 471, but there is also in the case of each pair a very faint band visible, or suspected, at the position of the blue band in the other pair. Further, in Arg. Oeltzen 17681, Vogel found the bright band sufficiently long to include both positions of the band.

One suggestion which presents itself is whether these bands, or, more correctly, these groups of bright lines, may be variable, so that, under certain conditions, one or other of them becomes brilliant. Such a state of things would reconcile our observations of $+37^{\circ}$ 3821 with the earlier measures of Dr. Copeland, and, indeed, might possibly explain, if this variability should be established, the circumstance that so accurate an observer as Professor Vogel did not detect, even with his smaller instrument in 1873, the very large difference of position of the band in 4001 from that of the corresponding band in the stars 4013 and 3956, which was so conspicuous in 1883, and is so still at the present time. In the broad characters of their spectra, and in their magnitudes, the Wolf-Rayet stars have remained unchanged since the discovery of their remarkable spectra in 1867.

As the only direct evidence of such a variability rests upon the change of position of the band in Dr. Copeland's star since his observation of it in 1884, I wrote to Dr. Copeland to ask if his position rested upon sufficiently accurate measures or was arrived at by estimation only. In reply he says:—"The place of the blue line (rather band) in D.M. $+37^{\circ}$ 3821, given in the 'Monthly Notices,' is a mere estimate to show the character of the star."

Whether any change of position of the band has taken place must therefore remain at present uncertain; but, independently of any such direct evidence of variability, the two positions of the very bright blue band, with the suspicion of faint bands at the alternate positions, appear to us suggestive of possible variation, especially when we

consider that the spectra of these stars consist of numerous absorption bands and groups of bright lines upon a feeble continuous spectrum, a character of spectrum which seems to point to a probably unstable condition of the atmospheres of these stars.

The large difference of position of the bands in the two groups of stars is much too great to admit of an explanation founded upon a possible orbital motion of the stars. Besides, the near coincidence of Dr. Copeland's measures of two bright lines common to the stars 4001 and 4013 shows that the difference of position of the blue band is not due to motion in the line of sight.*

If future observations should show that the bright blue groups are variable, we must look, it would seem, to causes of a physical or a chemical nature.

If the two bright groups, differing in position by about λ 0040, belong to different substances, or, less probably, perhaps, to different molecular conditions of the same substance, it is conceivable that one or other substance, or molecular state, may predominate and appear brilliant, according to certain unknown conditions which may prevail in the stars' atmospheres.

It might be suggested that both bands are due to a long group of bright lines, extending from about λ 461 to λ 471, and that this long group is cut down by absorption bands; in one pair of stars an absorption from the green cuts off the less refrangible part of the long group down to about λ 467, while in the other two stars the more refrangible part is eclipsed, and the bright group appears as in 4001.

The appearance of the spectra in our instruments scarcely seems to us to be in accordance with such a view, because, though we did suspect brightenings in the alternate places, the appearance of the spectrum was not such as to suggest a bright group dimmed by absorption, for in that case the amount of absorption needed to all but obliterate a group, as bright as it appears in the other pair of stars, would have blotted out completely the relatively feeble continuous spectrum. This continuous spectrum, though faint, was still distinctly seen.

More observations are needed, but it appeared to us desirable by these suggestions to invite the attention of observers to the points in question.

* Dr. Copeland permits me to give the following measures of the bright lines in the Wolf-Rayet stars, which were made by him and Mr. Lohse on January 28, 1884.

Star.	1st yellow line.	2nd yellow line.	Bright line.	Faint line.	Large blue band.
+ 35° 4001....	—	—	541·2(3)	522·0(1)	469·5(3)
+ 35° 4013....	582·4(2)	568·9(2)	541·0(2)	—	465·4(2)
+ 36° 3956....	581·9(2)	570·4(2)	—	523·3(1)	464·9(2)

As the main object of our examination of these stars was to determine whether the bright band in the blue was to be regarded as showing the presence of carbon by its coincidence with the blue band of the hydrocarbon flame, we were not able, from the pressing claims of other work, to extend our examination to many other points in connexion with the spectrum of these faint stars, for an exhaustive examination of which, indeed, our instruments are not sufficiently powerful.

We have stated already that the fairly luminous continuous spectrum reaches up to the bright band in all three stars, and extends beyond into the violet, as far as the eye could be expected to follow it.

The spectra are weakened at many points by what appear to be absorption bands, and are crossed by several brilliant lines, the positions of some of which have been given by Vogel and by Copeland.

An examination with spectroscope B of some of these bright lines, as they appear under small dispersion, showed them to be really not single lines, but short groups of closely-adjacent bright lines.

One of the brightest of these lines is found in the star No. 4013, at the position, according to Vogel, of λ 570.

Dr. Copeland's measure for this line is λ 568.9 in star 4013, and λ 570.4 in the star 3956.

As this position is not very far from that of the green pair of sodium lines at λ 5687 and λ 5681, it has been suggested that the line in the star is due to sodium, though there is no line of comparable brightness in the star's spectrum at the position of the dominant pair of the sodium spectrum at D.*

On confronting in spectroscope B the star line with the green sodium lines, the bright space in the star's spectrum was seen to consist of a short group of several bright lines close together, and nearly equally bright. This group appeared to extend through about four times the interval of the sodium pair, which would make the length of the group about λ 0024. The green sodium lines cross the group at about one-fourth to one-third of the length of the group from its more refrangible end. The group in the star is rather less bright at the two ends, but there is no gradual shading off in either direction, as in the case of a fluting.

When we examined this part of the spectrum with the small dispersion of a prism of 45° , we were pretty sure of a feeble bright line, less refrangible than the pair of bright groups in the yellow, and not far from the position of D. We were not able to see this line in spectroscope B with sufficient clearness to enable us to fix its position. It may be D, or, perhaps more probably D₃.

* The 570 line is most probably the green sodium line 569, the absence of the yellow sodium being explained by the half-and-half absorption and radiation mentioned in the discussion of the causes which mask and prevent the appearance of a line in a spectrum (Bakerian Lecture for 1888, 'Roy. Soc. Proc.,' vol. 44, p. 41).

In No. 4001, Vogel saw a line at the position of the F line of hydrogen. It is probable that this line, as is the case in so many stars in which it appears bright, is variable, as we were not able to see it when the $H\beta$ line from a vacuum-tube was thrown in. In the similar star D.M. +37° 3821, as we have stated already, the F line of hydrogen was very bright.

We were unable to detect in any of the stars a brightening of the spectrum at the position of the chief line of the bright-line nebulae. For this examination the lead line at λ 5004.5 was thrown in, and the continuous spectrum of the star near to this position carefully scrutinised.

In their original paper, Wolf and Rayet state that they were not able to detect any nebulosity about the stars. They say: "Elles ne présentent non plus aucune trace de nébulosité" (*loc. cit.*, p. 292).

In a recent paper, Mr. Keeler, of the Lick Observatory, confirms this view. He says: "At my request, Mr. Burnham and Mr. Barnard examined the Wolf-Rayet stars in Cygnus for traces of surrounding nebulosity, but with only negative result."*

Notwithstanding these negative results, it appeared to us of great interest to ascertain further if any nebulosity would come out in a photograph of the stars taken with a long exposure.

Mr. Roberts responded at once to our wish when we asked his invaluable assistance, and on November 1st, of this year, he took a photograph of this region of Cygnus, with an exposure of two hours.

The three stars come out strongly upon the plate, but there is no nebulosity to be seen near any of them. There are faint stars in close proximity to the three stars, and apparently surrounding them, and, in the case of No. 3956, six of these faint stars are seen close to it, in an apparent spiral arrangement.

Though this surrounding of faint stars should be pointed out, it should, at the same time, be stated that the whole neighbouring region is so densely studded with similar faint stars that it would be rash, perhaps, at present to suggest that this apparent connexion of the bright-line stars with faint ones near them may be other than accidental.†

* 'Publications of the Astronom. Soc. of the Pacific,' No. 11.

† [Mr. Roberts has furnished us with the following description of the stars as they appear on his photograph:—

"No. 4001 appears as a multiple star made up of one bright, two fainter, and one very faint star partly behind the others; there is also a fourth bright star close to the multiple star. The group is surrounded by at least eight faint stars within a radial distance of $\pm 86''$ of arc from centre to centre.

"No. 4013.—The photo-image of this star is made up of three stellar images touching each other in a line slightly curved. Two are bright and one faint; and there are indications of two other faint stars behind the two bright ones. This

Professor E. C. Pickering informs me "that photographs have been obtained at the Harvard College Observatory of all the stars hitherto discovered whose spectra consist mainly of bright lines and are of the class discovered by Rayet. Part of these have been photographed at Cambridge, and the remainder in Peru." He states that they may be divided into three sub-classes, according to the characters of their fifteen bright lines. He says, further: "Photographs of the spectrum of planetary nebulae have also been obtained. They resemble closely the spectra described above, except that the line 500 is strongly marked; 470 is seen in most of them, while the lines due to hydrogen are also bright."

It would seem that Professor Pickering's photographs do not permit him to distinguish the different positions of the bright blue band in some of these stars, for he gives for all the stars the same position, namely, λ 470.

We regret that the insufficiency of our instrumental means has left our examination of the spectra of these stars less complete than we could wish. Our observations appear to us, however, to be conclusive on the main object of our enquiry, namely, that the bright blue band in the three Wolf-Rayet stars in Cygnus, and in D.M. +37° 3821, is not coincident with the blue band of the Bunsen flame.

V. "On Stokes's Current Function." By R. A. SAMPSON, Fellow of St. John's College, Cambridge. Communicated by Professor GREENHILL, F.R.S. Received November 24, 1890.

(Abstract.)

In Maxwell's 'Electricity and Magnetism,'* a view is put forward, in accordance with which we may regard any irrotational motion in a perfect liquid, for which the velocity potential is a solid zonal harmonic, as due to the juxtaposition at the origin, and upon the axis of symmetry, of sinks and sources.

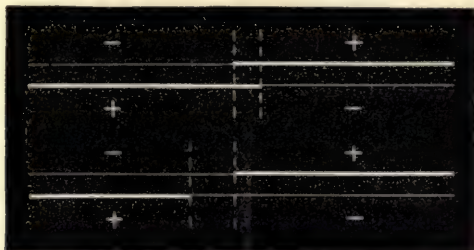
But, in a liquid, any irrotational motion which is symmetrical with respect to an axis gives a velocity potential which may be expressed as a sum of a series of solid zonal harmonics, their common axis being the axis of symmetry, and their origin arbitrary, provided multiple image of four or five stars is surrounded by five bright and seven faint stars; all within a radial distance of 82" of arc measured from centre to centre of the multiple star. The multiple image measures $\pm 55''$ in length and $\pm 19''$ in breadth.

"No. 3956.—Its photo-image is $\pm 27''$ in diameter. It is encircled by three stars of lesser brightness, and six faint ones within a radial distance of 59", i.e., there are nine stars within a radial distance of 59".—Dec. 5.]

* Vol. 1, chapter ix.

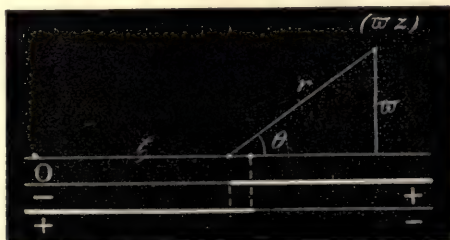
it is excluded from the region to which the expressions apply. The position of the origin upon the axis is arbitrary, since by a transference formula we may pass from one origin to another.

Let us now consider the system formed by a line source and a line sink, of equal strengths, extending along the axis from an arbitrary origin to infinity in opposite directions. Such a system I shall call an *extended doublet*, of strength m , where m is the strength per unit length of that part which lies on the positive side of the origin.



By the superposition of two extended doublets, of equal but opposite strengths, we can produce a sink or a source upon the axis. Hence, in a liquid, any irrotational motion which is symmetrical with respect to an axis, may be produced by superposition of extended doublets, whose origins depart but little from an arbitrary point on the axis of symmetry.

Now for an extended doublet of strength m , Stokes's current function ψ , for any point distant r from the the origin, is $-2mr$. For let ζ be the distance of the origin of the doublet from the origin of co-ordinates, and let $\psi(m, \zeta)$ be the value of Stokes's current function for



any point (w, z) . Then if $\delta\psi$ be the current function for a source of strength $2m\delta\zeta$, at the point ζ of the axis, we get

$$\frac{1}{\pi} \frac{d}{dr} \cdot \delta\psi = 0.$$

$$\frac{1}{\pi} \cdot \frac{d}{rd\theta} \cdot \delta\psi = -\frac{2m\delta\zeta}{r^2}.$$

Therefore
$$\frac{d}{d\theta} \cdot \psi = -2m \cdot \frac{\pi}{r} \cdot \delta\zeta$$

$$= -2m \cdot \sin \theta \cdot \delta\zeta.$$

Whence
$$\delta\psi = 2m\delta\zeta \cdot \cos \theta,$$

disregarding a constant.

But
$$\psi = \psi(m, \zeta) + \psi(-m, \zeta + \delta\zeta),$$
and clearly,
$$\psi(-m, \zeta + \delta\zeta) = -\psi(m, \zeta + \delta\zeta).$$

Hence
$$\psi(m, \zeta) - \left[\psi(m, \zeta) + \frac{d\psi}{d\zeta} \delta\zeta \right]$$

$$= \frac{d\psi(m, \zeta)}{d\zeta} \delta\zeta$$

$$= 2m\delta\zeta \cdot \cos \theta$$

$$= 2m\delta\zeta \cdot \frac{z - \zeta}{\sqrt{\{ \pi^2 + (z - \zeta)^2 \}}}.$$

Therefore
$$\frac{d\psi}{d\zeta} = 2m \cdot \frac{z - \zeta}{\sqrt{\{ \pi^2 + (z - \zeta)^2 \}}},$$

and
$$\psi = -2m\pi \dots \dots \dots (1),$$

where
$$r = \sqrt{(\pi^2 + z - \zeta^2)},$$

disregarding a constant.

Thus if $m = f(\zeta) d\zeta$, we may, by properly choosing the function f , write

$$\psi = \int_{-\infty}^{+\infty} f(\zeta) \sqrt{\{ \pi^2 + (z - \zeta)^2 \}} d\zeta \dots \dots \dots (2),$$

where ψ is the current function for any irrotational motion in a liquid, symmetrical about the axis of z .

Again, if
$$r = \sqrt{\{ \pi^2 + (z - \zeta)^2 \}},$$

$$\frac{dr}{d\pi} = \frac{\pi}{r}, \quad \frac{dr}{dz} = \frac{z - \zeta}{r},$$

$$\frac{d^2r}{d\pi^2} = \frac{1}{r} - \frac{\pi^2}{r^3}, \quad \frac{d^2r}{dz^2} = \frac{1}{r} - \frac{(z - \zeta)^2}{r^3}.$$

Therefore
$$\frac{d^2r}{d\pi^2} + \frac{d^2r}{dz^2} = \frac{2}{r} - \frac{\pi^2 + (z - \zeta)^2}{r^3}$$

$$= \frac{1}{r}$$

$$= \frac{1}{\pi} \frac{dr}{d\pi},$$

and the expression (1), and consequently also (2), satisfies the differential equation

$$\frac{d^2\psi}{d\varpi^2} + \frac{d^2\psi}{dz^2} - \frac{1}{\varpi} \frac{d\psi}{dz} = 0 \dots\dots\dots (3),$$

or, as I shall write it, $D\psi = 0$.

When the motion is rotational, (3) no longer holds. In fact, as is well known, we have under all circumstances

$$\frac{1}{\varpi} D\psi = -2\omega,$$

where ω is the resultant molecular rotation at the point (ϖ, z) .

Thus, if there is molecular rotation in the fluid, (3) is replaced by

$$\frac{d^2\psi}{d\varpi^2} + \frac{d^2\psi}{dz^2} - \frac{1}{\varpi} \frac{d\psi}{dz} = -2\varpi\omega \dots\dots\dots (3a).$$

Again, if ∇^2 stand for the operator $\frac{d^2}{d\varpi^2} + \frac{d^2}{dz^2} + \frac{1}{\varpi} \frac{d}{d\varpi} + \frac{1}{\varpi^2} \frac{d^2}{d\phi^2}$, ϕ being the azimuthal angle about the axis of symmetry, it may be seen at once that

$$D\psi = \frac{\varpi}{\sin\phi} \nabla^2 \frac{\sin\phi}{\varpi} \psi \dots\dots\dots (4).$$

Consequently (3a) may be written

$$\begin{aligned} \nabla^2 \frac{\psi \sin\phi}{\varpi} &= -2\varpi\omega \times \frac{\sin\phi}{\varpi} \\ &= -2\omega \sin\phi \dots\dots\dots (3a'). \end{aligned}$$

Consequently

$$\psi = \psi_0 + \frac{\varpi}{2\pi \sin\phi} \iiint \frac{\sin\phi \omega \, dx \, dy \, dz}{r},$$

where ψ_0 is a solution of (3).

Or ψ consists of a solution of (3) together with $\frac{\varpi}{2\pi \sin\phi} \times$ the potential at the point considered of a distribution of mass of density at any point $\sin\phi \times$ the molecular rotation at that point. This result is given by Basset, 'Hydrodynamics,' vol. 2, § 306.

I give one other general result. Since

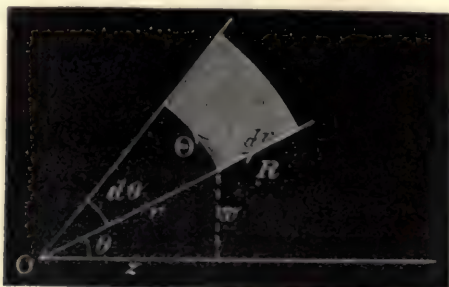
$$\omega = -\frac{1}{2\varpi} D\psi \dots\dots\dots (3a),$$

the circulation in any evanescent circuit drawn in a meridional plane is

$$-\iint \frac{1}{\varpi} D\psi \, d\varpi \, dz \dots\dots\dots (5),$$

where the integration extends over the area embraced by the circuit.

This result enables us to transform $D\psi$ readily from cylindrical to other systems of coordinates. For instance, consider polar coordinates, r, θ , and let us find the circulation in a small rectangle bounded by $r, r+dr, \theta, \theta+d\theta$.



Let the velocities in the direction of r and perpendicular to it be R, Θ . Then the circulation in this circuit is

$$Rdr + \left[\Theta r + \frac{d}{dr} (\Theta r) dr \right] d\theta - \left[R + \frac{dR}{d\theta} d\theta \right] dr - \Theta r d\theta$$

$$= r dr d\theta \left[\frac{d\Theta}{dr} + \frac{\Theta}{r} - \frac{dR}{rd\theta} \right].$$

Now $\Theta = -\frac{1}{r \sin \theta} \cdot \frac{d\psi}{dr},$

$$R = \frac{1}{r^2 \sin \theta} \cdot \frac{d\psi}{d\theta}.$$

Thus the expression in square brackets is—

$$-\frac{1}{r \sin \theta} \left[\frac{d^2 \psi}{dr^2} + \frac{1}{r^2} \left(\frac{d^2 \psi}{d\theta^2} - \cot \theta \frac{d\psi}{d\theta} \right) \right],$$

or

$$D\psi = \frac{d^2 \psi}{dr^2} + \frac{\sin \theta}{r^2} \frac{d}{d\theta} \left(\frac{1}{\sin \theta} \frac{d\psi}{d\theta} \right)$$

$$= \frac{d^2 \psi}{dr^2} + \frac{1-\mu^2}{r^2} \frac{d^2 \psi}{d\mu^2} \dots\dots\dots (6),$$

if μ stands for $\cos \theta$.

Other applications will be found later.

Reverting now to the expression (1), it will be seen that the direct distance of any point from a point on the axis of symmetry plays the same part in the theory of Stokes's current function that is played by its reciprocal in the theory of the potential function belonging to symmetrical distributions of matter.

Thus if $r_0, 0, r, \theta$, be the coordinates of a point upon the axis, and of any other point, the distance between these points, $\sqrt{(r_0^2 - 2r_0r \cos \theta + r^2)}$, may be developed in a convergent series, say

$$\sum_{n=0}^{\infty} -\frac{r^n}{r_0^{n-1}} I_n(\cos \theta) \quad \text{or} \quad \sum_{n=0}^{\infty} -\frac{r_0^n}{r^{n-1}} I_n(\cos \theta),$$

according as r_0 is greater or less than r , $I_n(\cos \theta)$ being a certain function of θ , and we see from (6) that

$$(1 - \mu^2) \frac{d^2 I_n(\mu)}{d\mu^2} + n(n-1) I_n(\mu) = 0 \dots\dots\dots (7).$$

Now it is evident from the analogue of zonal harmonics that it is proper to discuss the function $I_n(\cos \theta)$, and other solutions of (7) before considering the applications of Stokes's current function to the motion of liquids. It is with this discussion that the first three chapters are occupied, and, as might be expected, the theory closely resembles that of spherical harmonics. I have accordingly made free use of the order and methods adopted by Heine in his '*Handbuch d. Kugelfunctionen*,' more especially in chapters i and ii,* where the necessary changes were slight. Moreover, the functions I deal with have themselves been discussed by Heine, on a different method, and most of the expressions which I find in the following pages are given by him. Full references to these are given in §18.

The idea of developing the solutions of $D\psi = 0$ in a manner more or less analogous to that employed with regard to Laplace's equation appears to have been first used by O. E. Meyer,† who obtains the equation (7), shows that the functions contain $1 - \mu^2$ as a factor, and that they obey (28), chapter ii. An expression which shows the relation of the functions to zonal harmonics was given by Mr. Butcher;‡ and functions of fractional order have been used by Mr. Hicks,§ in connexion with his researches on the theory of the motion of vortex rings. The fuller account of such functions which is found in the following pages may be of interest in relation to these; for example, I would refer to §63, chapter v.

* The following sections of the first three chapters contain methods or results which, so far as I am aware, are original:—12, 13, 17, 21, 25, 26, 29, 30, 32, 36, 38, 40, 42. The remainder of the paper is original, except where specially acknowledged, or where a result is too well known for that to be necessary.

† '*Crelle*,' vol. 73, 1871.

‡ '*London Math. Soc. Proc.*,' vol. 8. See p. 143, chapter vi.

§ '*Phil. Trans.*,' 1884, 1885.

The applications to hydrodynamics which I here give are of mathematical interest rather than physical. They are chiefly in connexion with the motion of viscous liquids. In 'Crelle-Borchardt,' vol. 81, 1876, Oberbeek has given the velocities produced in an infinite viscous liquid by the steady motion of an ellipsoid through it, in the direction of one of its axes, and from these Mr. Herman* has found the equation of a family of surfaces containing the stream lines relative to the ellipsoid. In chapter vi, Stokes's current function is obtained by a direct process for the flux of a viscous liquid past a spheroid, and it is shown that the result differs only by a constant multiple from the particular case of Mr. Herman's integral.

Some minor applications are also given, namely, the solutions are obtained for flux past an approximate sphere, and past an approximate spheroid. The solution is also obtained for flux through a hyperboloid of one sheet, where it appears that the stream surfaces are hyperboloids of the confocal system. A particular case is that of flux through a circular hole in a wall, and this is interesting because we see that, by supposing internal friction to take place in the liquid, we find an expression which gives zero velocity at the sharp edge, and thus avoids the difficulty which is always present in the solution of such problems on the supposition that the liquid is perfect. A comparison may be instituted between this problem, and that of the effect of a disturbing periodic force upon a dynamical system capable of vibrating alone with a period equal to that of the force. It is well known that the amplitude of the vibration induced appears infinite, if we totally disregard friction, and this difficulty is met by the fact that the damping effect of even slight friction is rendered considerable by high velocities. Now a viscous liquid can move irrotationally, and, if there were no friction at the boundaries, this is the class of motion it would take in cases of flux past or through obstacles. But if the obstacle terminated in a sharp edge, this would make the velocity there infinite, and the friction, however inconsiderable elsewhere, would here become of account. The boundary conditions which were necessary for the existence of irrotational motion throughout the liquid would no longer apply, and the whole character of the solution would be changed. This would at any rate seem to apply to cases in which the whole motion is slow, and when, consequently, the boundary conditions which must hold are pretty well understood.

The paper concludes with an attempt to discuss the flux past a spheroid, or through a hyperboloid at whose boundary there may be slipping. The current function is not obtained, all that appears being that it probably differs from the parallel case of the sphere in being far more complicated than when there is no slipping. From

* 'Quart. Jour. Math.,' 1889 (No. 92).

this we except the case of the flux through a circular hole in a plane wall, when the solution for no slipping satisfies the new conditions.

Presents, December 11, 1890.

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December 18, 1890.

Lieut.-General STRACHEY, R.E., Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

- I. "On a Determination of the Boiling Point of Sulphur, and on a Method of Standardising Platinum Resistance Thermometers by reference to it." By HUGH L. CALLENDAR, M.A., Fellow of Trinity College, Cambridge, and E. H. GRIFFITHS, M.A., of Sidney Sussex College, Cambridge. Communicated by J. J. THOMSON, F.R.S., Cavendish Professor of Physics. Received November 29, 1890.

(Abstract.)

Experiments by different observers have shown that electrical resistance thermometers afford the most convenient and accurate method of measuring temperature through a very wide range. By selecting a particular thermometer as the standard, and directly comparing others with it, it has been found possible to attain a degree of accuracy of the order of $0\cdot001$ in the relative measurements between 0° and 100° C., and of the order of $0\cdot01$ at 450° C.

In a previous communication* it has been shown that, if t be the temperature by air thermometer, and if pt be the temperature by platinum resistance thermometer, the difference between them is very closely represented from 0° to 700° C. by the formula

$$d = t - pt = \delta \{ \overline{t/100}^2 - t/100 \} \dots\dots\dots (d).$$

The value of the constant δ for a particular wire was found to be 1.570.

The object of the present paper is to describe a method of finding the value of this constant for any such thermometer, by means of a single observation at some known fixed point other than 0° or 100° C.

The boiling point of sulphur happens to be the most convenient for this purpose. We have therefore made a careful determination of this point by reference to the standard air thermometer, and have given a full description of the method and apparatus which we have

* Callendar, 'Phil. Trans.,' A, 1887, p. 161.

found most suitable for standardising platinum thermometers by means of it.

The paper is divided into three parts.

Part I contains a description of the method and apparatus employed in comparing the platinum thermometers used in this investigation with the air thermometer at a temperature very near the boiling point of sulphur.

Part II contains the determination of the actual boiling point of sulphur by means of the thermometers thus standardised, and a description of the method and apparatus to be used in standardising other platinum thermometers. A table is also given reduced from a previous series of observations of other fixed points which may be used for the same purpose.

Part III contains a comparison of the platinum and air thermometers between 0° and 100° , and shows that the δ -formula holds accurately between those limits.

The determination of the boiling point of sulphur was made by means of three platinum thermometers, L, M_1 , and M_2 , constructed out of the wire used in the experiments of 1887, before referred to.

Full descriptions of these thermometers are given in the paper. They were furnished with double electrodes for measuring the resistance of the connecting wires at each observation, their insulation was carefully tested, and all due precautions were taken to guard against thermal effects and other sources of error.

Thermometers M_1 and M_2 were standardised by direct comparison with an air thermometer at the boiling point of sulphur. Full particulars are given of the details of the observations and calculations, showing the limits of error of the experiments.

The expansion of the glass forming the bulb of the air thermometer was determined both by the method of linear expansion, and also by using the bulb itself as a mercury weight thermometer. The values found by the two methods agreed very closely.

The small changes of the volume of the bulb were determined from time to time during the progress of the experiments. The final observations were not taken till the thermometer had reached a fairly steady state.

The limit of accuracy attainable with this air thermometer was found to depend chiefly on that of the barometric readings. The barometer used was therefore verified by a careful comparison with the standard metre scale.

The iron-tube apparatus in which the platinum and air thermometers were compared was so constructed as to be capable of being maintained at a constant temperature by a steady flow of sulphur vapour for any length of time.

Observations were taken with it on two separate days. On each

occasion the temperature was kept steady to $0^{\circ}\cdot 1$ for about two hours. Allowing for the difference of the atmospheric pressure, the temperature attained was the same on both days.

The results of the comparison were in perfect agreement with the experiments of 1887, and showed that the δ -coefficient of the wire had not altered appreciably in the interval.

The apparatus which we have found most convenient for standardising platinum thermometers by means of the boiling point of sulphur consists of a wide glass tube, 40 cm. long and 4 cm. in diameter, with a spherical bulb at the end. Tubes of this kind are commonly used to heat Victor Meyer's vapour-density apparatus. For brevity we have called it a "Meyer" tube.

The outside of the tube is thickly padded with asbestos wool, with the exception of the lower half of the bulb, and of a short length of 3—5 cm. at the top, which serves as a condenser. The tube is filled with sulphur to a level of 3 or 4 cm. above the bulb, and is heated by a Bunsen burner. The gas is adjusted so as to keep the level of the vapour near the top of the tube, which is covered with asbestos card to prevent the sulphur catching fire.

Our experiments have shown that a thermometer inserted in an apparatus of this kind will not attain the actual temperature of the vapour, unless it is protected from radiation to the sides of the tube, and from the condensed liquid which runs down the stem. The lowering of temperature due to radiation, &c., may readily amount to upwards of 2° at the boiling point of sulphur.

The method which we have adopted for screening the thermometer is to bind an umbrella of asbestos card on to its stem a short distance above the bulb. Two coaxial tubes are hung on to this umbrella to screen the thermometer from radiation. We have found that glass is not sufficiently opaque to heat radiation at this temperature. The inner tube at least should be of metal.

To avoid superheating of the vapour, it is necessary to make sure that the level of the liquid sulphur stands well above that part of the bulb which is exposed to the flame.

Using these precautions, we have found that the temperature by normal air thermometer at constant pressure of the saturated vapour sulphur boiling freely under a pressure, of 760 mm. of mercury at 0° C., and $g = 980\cdot 61$ C.G.S. (sea level in lat. 45°), is

$$t = 444^{\circ}\cdot 53 \text{ C.}$$

The value given by Regnault* is nearly 4° higher than this; but in the account which he gives of his experiments he has pointed out several sources of error, and it is evident that he did not place much confidence in his results.

* 'Mémoires de l'Institut,' vol. 26, p. 526.

The close agreement between the air thermometer experiments of 1887 and the present series, leads us to conclude that the number above given is probably correct to a tenth of a degree, and that it may be safely used for standardising platinum thermometers.

The method which we recommend for standardising platinum thermometers is briefly as follows:—Observe the value R_s of the resistance in sulphur vapour in an apparatus such as we have described. Calculate the value of pt_s by the formula

$$pt_s = 100 (R_s - R_0) / (R_{100} - R_0).$$

Find the temperature t of the sulphur vapour, corresponding to the corrected barometric pressure H_0 , from the formula

$$t = 444.53 + 0.082 (H_0 - 760).$$

The appropriate value of δ is then given by the equation

$$t - pt = \delta \{ \sqrt{t/100}^2 - t/100 \}.$$

We have made use of this method to reduce the results given in a previous communication, "On the Determination of some Boiling and Freezing Points by means of the Platinum Thermometer,"* and we find that the values of t deduced from the observations with several thermometers of different patterns and with very different coefficients, are in remarkably close agreement. The results found with the three best thermometers are given in the following table:—

Table of Boiling and Freezing Points reduced by Formula (d).

Nature of experiment.	Thermometers used.			Mean.
	E.	F.	G.	
B.p. of aniline (760 mm.)	184.11	184.13	184.14	184.13
„ naphthalene „	217.88	217.96	217.98	217.94
„ methyl salicylate „	222.98	223.08	—	223.03
„ benzophenone „	305.82	305.87	305.78	305.82
„ triphenyl methane (770.8 mm.)	356.47	—	356.41	356.44
„ mercury (760 mm.)	356.74	356.82	356.71	356.76
Freezing point of tin	231.66	231.66	231.73	231.68
„ bismuth	269.18	—	269.25	269.22
„ cadmium	320.70	—	320.66	320.68
„ lead	327.66	—	327.71	327.69
„ zinc	417.55	—	417.59	417.57

The fixed points given in the above have not been so carefully determined as the boiling point of sulphur. They rest entirely on the

* See Griffiths, 'Phil. Trans.,' A, 1891.

assumption of the accuracy of the δ -formula, and have not been directly referred to the air thermometer. We believe, however, that they are probably correct to $0^{\circ}1$ C., and that they may be safely used to standardise thermometers of limited range, in cases where it may happen to be inconvenient to make use of the sulphur point.

In comparing the platinum and air thermometers between 0° and 100° C. observations were taken at intervals of 5° all the way up. The mean deviation of the observations from the parabolic formula (d) is only $0^{\circ}006$. This corresponds to the limit of accuracy of the barometric readings, and there is no reason to suppose that the δ -formula may not represent the difference even more closely than this.

The same platinum thermometer has been compared with several mercury thermometers standardised at Kew.* The result seems to show that the Kew standard reads $0^{\circ}1$ C. lower than our air-thermometer at 30° .

II. "On the Generic Identity of *Sceparnodon* and *Phascolonus*."

By R. LYDEKKER, B.A. Communicated by Professor W. H. FLOWER, C.B., F.R.S. Received November 19, 1890.

[PLATE 1.]

In the year 1872, Sir Richard Owen described and figured in the 'Phil. Trans.'† two imperfect lower jaws of a large extinct Wombat, from the Pleistocene of Queensland, under the name of *Phascolomys* (*Phascolonus*) *gigas*, the term *Phascolonus* being employed in a sub-generic sense. The species *Phascolomys gigas*, it should be observed, was founded by the same writer‡ at an earlier date, upon the evidence of a detached cheek-tooth. Subsequently Sir Richard Owen§ described and figured certain imperfect upper incisors, from Queensland and South Australia, characterised by their peculiarly flattened and chisel-like shape, under the new generic name *Sceparnodon*, which was suggested from their contour.

In cataloguing the fossil Mammalia in the collection of the British Museum,|| I was at once struck by the circumstance that, while the upper incisors of the so-called *Phascolomys gigas* were unknown, there were no cheek-teeth which could be referred to *Sceparnodon*, and it accordingly occurred to me that the two might prove to be identical. Support was afforded to this conjecture by the following circumstances:—1st. The incisors of *Sceparnodon* agreed fairly well in relative size

* Griffiths, 'Brit. Assoc. Report,' 1890.

† Page 257, Pl. 36—38, 40.

‡ 'Encyclopædia Britannica,' 8th ed., vol. 17, p. 175 (1859).

§ 'Phil. Trans.,' 1884, p. 245, Pl. 12.

|| 'Cat. Foss. Mamm. Brit. Mus.,' pt. 5, pp. 157—159 (1887).

with the jaws and cheek-teeth of *Phascolomys gigas*. 2nd. The incisors of *Scepharnodon* were decidedly of a Wombat-like type, differing mainly from those of existing Wombats by their large size and excessive flattening and expansion. 3rd. One of the incisors of *Scepharnodon* agreed so closely in the structure of the enamel, and the reddish stains upon the same, with an upper premolar of *Phascolomys gigas*, that I even suggested both teeth might have belonged to the same individual animal.

As the result of the above it was concluded that the teeth described as *Scepharnodon Ramsayi* were probably the upper incisors of *Phascolomys gigas*, and on this supposition it was considered that the latter was generically distinct from existing Wombats, and it was accordingly entered as *Phascolonus gigas* in the Museum catalogue.

Thus the matter stood till a short time ago, when I visited the Exhibition of the Mineralogical Products of New South Wales, recently held at the Crystal Palace. Among the specimens exhibited was a small collection of Mammalian remains, obtained from clay beds, near Miall Creek, in the neighbourhood of Bingera, a station lying close to the northern border of New South Wales. These deposits, which have only recently been brought to notice, and appear to be full of Mammalian remains, have been described by Mr. W. Anderson, in the 'Records of the Geological Survey of New South Wales.* All the bones from these beds are of a characteristic pale-brown colour, by which they can be distinguished at a glance from those of all other Australian deposits. The following account is taken from Mr. Anderson's report, in order to give an idea of the richness of these ossiferous deposits. This writer observes that "several tons of bones were recovered, but the majority of them were more or less broken, although many perfect specimens were procured. Those which occurred most frequently in the deposit were the long bones of the limbs, the small bones of their distal extremities, and vertebræ, and, as a rule, these were also the most perfect, and in the best state of preservation. Rib bones were rather common, but were mostly broken into short fragments. Of the vertebræ, specimens of the axis and atlas were usually found entire, both in the case of those which had belonged to the smaller animals, such as the Kangaroo (*Macropus*), &c., and also to the larger forms, such as *Diprotodon*, &c. Generally, however, only the bodies of the other vertebræ remained, the spines and the various processes having been broken off. Jaws, both large and small, occurred frequently, while isolated teeth were very abundant. The lower jaws of the larger forms were more frequently met with than the upper; indeed, the latter were rather rare. When they did occur, however, they never formed part of an entire cranium, but always consisted of the upper maxillary and palatal

* Vol. 1, pp. 116—225 (1889).

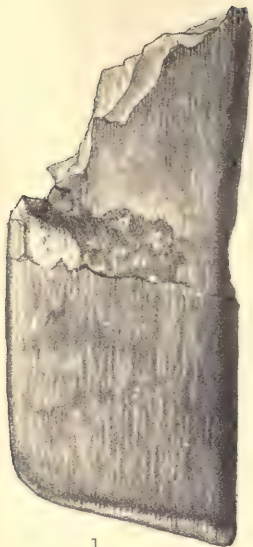
bones only, the rest of the cranium being absent. So far as I saw, there was no specimen that could be demonstrated to be a portion of the cranium proper of one of the larger animals, although there are undoubtedly small fragments of the cranial bones among the collection, which has, however, not yet been thoroughly examined. The bones of the pelvis were of rare occurrence, fragments of the thickest part of the os innominatum, about the rim of the acetabulum, being the part generally met with. In one instance, however, a very large portion of the pelvis of one of the larger animals was found, consisting of the greater part of one os innominatum, and the sacrum. Nearly perfect specimens of large scapulæ were in a few cases obtained, while fragments of the scapulæ of smaller animals, generally consisting of the articular head of the bone, with a portion of the neck and the coracoid process, the blade being wanting, were of frequent occurrence. The remains of birds, although by no means common, were often met with.

"There can be little doubt as to the comparative age of this ossiferous deposit. From the presence of pebbles of Tertiary basalt and tachylite, and the fact that the whole series rests upon the Tertiary basalt of the district, its origin is certainly of post-Tertiary date. The thickness of the series, the occurrence in the deposit of angular, as well as rounded water-worn pebbles, together with the relation which the whole series bears to the general level of the country, all point to the supposition that it more probably belongs to the Pleistocene than to the Recent period."

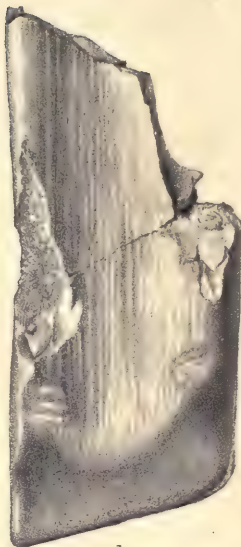
The series of specimens from those deposits shown in the Exhibition, which may be taken as a fair sample of the whole, comprises various remains of *Procoptodon* (the *Macropus* of Mr. Anderson), *Diprotodon*, *Nototherium*, molars and jaws of *Phascolonus*, and a large number of the incisors described as *Sceparnodon*. It will thus be apparent that if *Sceparnodon* were a distinct genus, it would be represented only by upper incisors, while *Phascolonus* would be equally deficient in these teeth. It is further noteworthy that all the Mammalian remains in the collection appear to belong to extinct genera, there being no evidence of the numerous species of *Macropus* and *Phascolomys*, which are so common in the Pleistocene of Queensland. This feature suggests that the Bingera deposits are somewhat older than those of the area last named.

Seeing what an important bearing the remains from Bingera have on the question of the identity of *Sceparnodon* and *Phascolonus*, I requested permission from the Commissioners of the Exhibition to borrow some of the specimens so named, a request which was at once most courteously acceded to.

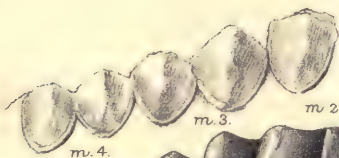
In due course I received from the Commissioners part of the right ramus of the mandible of *Phascolonus*, together with three imperfect



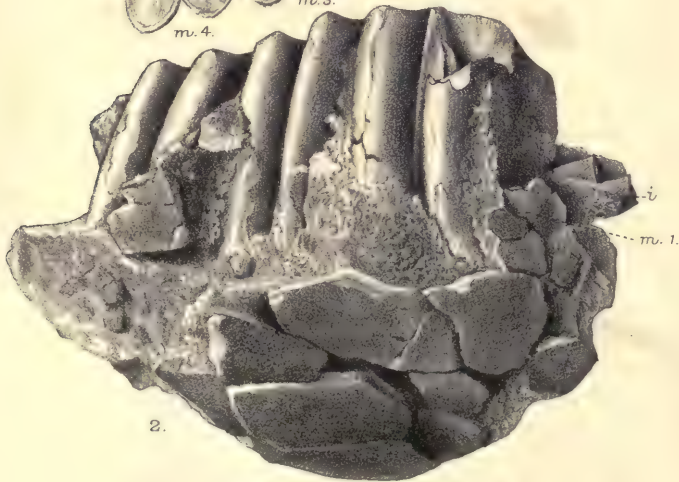
1.



1, a.



2, a.



2.



incisors of the so-called *Scepharnodon*. Of these, the lower jaw and the best preserved of the upper incisors are figured in the accompanying Plate.

The lower jaw (Plate 1, figs. 2, 2a) has been much crushed, and appears to have belonged to an individual just attaining maturity. It contains the third and fourth molars in a perfect condition, the second molar somewhat damaged, part of the root of the first molar, and the base of the incisor. The fragment agrees in all respects with the nearly complete ramus figured in the 'Phil. Trans.' for 1872, Pl. 36, 37, and, like the latter, shows that *Phascolonus* differs from living Wombats in the relatively smaller size of the last molar, more especially as regards its second lobe. The Queensland specimen also shows that the mandibular symphysis of the extinct form was much larger, and also relatively wider at its anterior extremity, than in *Phascolomys*. These features alone would, perhaps, be sufficient to justify the generic separation of the extinct form as *Phascolonus*, and the great width of the anterior part of the symphysis is especially significant, as being apparently adapted to fit with the wide upper incisors described as *Scepharnodon*.

The fragment of the upper incisor represented in figs. 1, 1a, belongs to the right side, and accords closely with the specimens figured in the 'Phil. Trans.' for 1884, Pl. 12, as the types of *Scepharnodon*, although it is still wider than either of those examples. The cutting-edge is entire, and exhibits the same oblique bevelling on the posterior surface that is shown in two of the specimens figured by Sir R. Owen. Both the anterior and posterior surfaces are covered by a coating of cement, and while there is a well-marked layer of enamel on the anterior surface, on the opposite aspect this element is either totally wanting, or reduced to a rudiment. The structure and colour of the cement, enamel, and dentine agree in all respects with those of the molars in the lower jaw of *Phascolonus*, and, allowing for their greater relative width and flattening, the upper incisors accord in proportionate size with the lower molars, as deduced from a comparison with a recent Wombat.

With the foregoing circumstantial evidence before us, I therefore venture to consider that we are now justified in definitely regarding the so-called genus *Scepharnodon* as based upon the upper incisors of the gigantic extinct Wombat known as *Phascolonus*. From the great width of the upper incisors as compared with the lower ones, it is pretty evident that the former must have worked somewhat obliquely against the latter. An interesting question arises as to the nature of the food which these excessively wide, and apparently fragile, chisel-like, upper incisors were adapted to cut; but the answer to this question I must leave to those intimately acquainted with the recent and Pleistocene flora of Australia.

DESCRIPTION OF PLATE 1.

Remains of *Phascolonus gigas*.

Figs. 1, 1a.—Anterior and posterior aspects of an imperfect upper incisor.

Figs. 2, 2a.—Outer and oral aspects of a fragment of the right mandibular ramus.

m. 1, m. 2, m. 3, m. 4, molars; i, incisor.

All the figures three-quarters natural size.

III. "Contribution to the Study of the Vertebrate Liver." By
SHERIDAN DELÉPINE, M.B. Edin. Communicated by T.
LAUDER BRUNTON, M.D., D.Sc., F.R.S. Received No-
vember 20, 1890.

(Abstract.)

Preliminary Remarks.—The following observations were made at the end of last year in the course of an investigation touching the action of drugs on cellular structure carried out by Dr. Lauder Brunton and myself, for the Royal Society.

Arrangement of the Hepatic Columns in a Classical Liver Lobule.—The following arrangement is visible in a plane perpendicular to the direction of the terminal vessels occupying the centre and the periphery of such a lobule. The columns of cells extend radially round the hepatic veins only in the direction of the portal veins, that is, in three, four, or five directions at most. In the intermediate region the columns present a typical feathery arrangement. The line from which the columns diverge will be called hereafter *hepatic line of divergence*. A similar arrangement is found around the terminal portal veins, giving rise to what I call *portal lines of divergence*.

Towards the *portal lines of divergence* the columns of cells become smaller in diameter, and join each other, becoming continuous with narrow tubes lined with flat epithelium and having the character of *intermediate tubes*. These narrow channels open into more distinct *terminal bile ducts*.

Arrangement of the Bile Canaliculi.—The liver columns branch from the portal lines of divergence towards the hepatic lines. This branching is, however, generally obscured by lateral anastomoses, but it becomes more evident when the bile canaliculi are distinct.

Two sets of bile canaliculi may be recognised:

1. The *main canaliculi*, occupying the axis of the columns of cells and becoming comparatively wide in the portal zone; it is the branching of these which renders that of the columns so evident in some specimens.

2. The *lateral canaliculi*, which pass between the cells forming the walls of the main canaliculi.

In addition to these two sets of passages an *intracellular branched system of lacunæ* may be described as forming the rootlets of the canaliculi. These spaces open directly into the canaliculi, and have been previously partly described by Pflüger and Kupffer.

Description of a True, Secretory, or Primary Lobule.—From what precedes it follows that the liver tubes, instead of being grouped round the terminal hepatic veins, are distinctly arranged in *small pyramidal masses, which correspond to the lobules of other glands*. These lobules are composed of the tubules diverging from the intermediate tubes found in the portal line of divergence, each set of intermediate tubes opening into a terminal bile duct. An arrangement somewhat analogous had already (in 1882) been supposed to exist by Sabourin, but he had been unable to discover in healthy livers what he believed to exist and had been obliged to fall back upon diagrammatic representations which are not altogether correct.

Development of the Liver.—Eberth and other observers since have recognised that the embryonic liver is composed of *hypoblastic tubes* branching in a mass of *mesoblast*. This being common to the liver and all other glands does not explain the differences between these organs. Between the third and sixth weeks of embryonic life (in man) nearly the whole of the mesoblastic tissue separating the hypoblastic columns becomes transformed into embryonic veins full of blood. In other glands only a small part of that tissue becomes transformed into veins, the greater part remaining in the shape of interlobular and interlobar tissue. In the liver, therefore, the hepatic veins take the place of the greater part of the stroma of other glands.

It is only around the oldest hypoblastic tubes which become ultimately ducts, and at the periphery of the organ, that a little mesoblastic tissue becomes transformed into fibrous connective tissue (Glisson's capsule).

Structure of the Hepatic Cells.—The *mitoma* gives unmistakable evidence of a further differentiation of parts of the trabeculæ composing it. These trabeculæ are contractile.

The *paramitoma* is enclosed in the meshes formed by the mitoma, and is the chief seat of the anabolic and katabolic changes taking place in protoplasma. The products thus formed probably under the influence of the mitoma accumulate in the midst of the paramitoma; when soluble, they permeate it; when insoluble, they are precipitated in the shape of drops or vacuoles, globules, granules, crystals. For these products only the name of *Paraplasma* should be reserved.

In the paraplasma two kinds of elements may be recognised, namely:—(1) those resulting from the katabolic process of the cell or other tissues (kataplasma), *e.g.*, bile pigment; (2) those resulting from the *anabolic processes* (anaplasma), *e.g.*, glycogen. In the

paramitoma of the liver cells the following anaplastic and kataplastic products may be demonstrated easily:—serous fluid (vacuoles) bile pigment, pigment containing iron, glycogen, fat, &c.

[It is to be remembered that, soon after the discovery of lobules in the pig's liver by Wepfer (1664), Malpighi (1666) described these lobules as being appended to the extremities of the vessels contained in Glisson's capsule. Ferrein (1749) showed that the liver, like other glands of the body, had a tubular structure; but, as he included the spleen among the tubular glands, it may be doubted whether he did more than generalise on the basis of his observations on the kidney. Three years before the publication of Kiernan's paper, Müller seems to have noticed the pinnate arrangement to which I have given the name of "portal lines of divergence;" but there can be little doubt as to the general acceptance of Kiernan's views after the publication of his observations in 1833 ('Philosophical Transactions'). The work of Kiernan was in great part based on the result of injections through the vessels.

For an account of the history of the subject, I would refer the reader to Kiernan's admirable paper, in which a great many points which I have purposely left aside will be also found mentioned.—Dec. 17, 1890.]

IV. "On certain Conditions that modify the Virulence of the Bacillus of Tubercle." By ARTHUR RANSOME, M.D., F.R.S. Received November 29, 1890.

It is acknowledged by most pathologists that tubercular sputum, dried up and broken into dust, is the most common vehicle by which the bacillus of tubercle is conveyed into the body.

But its power for evil is obviously modified by a multitude of conditions, some of them inherent in the animal body exposed to infection, others due to external influences. Judging from the facts relating to the distribution of tubercular disease, its incidence in certain localities, and especially its prevalence in badly drained, badly ventilated, and imperfectly lighted dwellings, it has been surmised that the three chief external conditions that mitigate the virulence of the bacillus are: (1) a dry soil (2) abundance of fresh air, and (3) light.

But hitherto, few, if any, direct experiments have been made to determine the extent to which these several influences possess mitigating powers.

It is true that Dr. Candler, in his work on the Prevention of Consumption, affirms that light is the chief agent in destroying the

bacillary virus, and Professor Koch, in his address to the International Congress at Berlin this year, lends the weight of his great authority to the same opinion; but, in neither case, is any proof given of the truth of this view.*

It was in order to test the influence of light, air, and dry soils upon the virulence of the bacillus of tubercle that the following series of experiments were devised.

It was decided to expose tuberculous sputum :—

(a) In a locality (Bowdon) where the soil was dry and sandy (about 100 feet in thickness) and where very few cases of phthisis were known to have originated. It was to be placed in full daylight or sunlight, and exposed to abundant streams of fresh country air.

(b) A portion of the same sputum would be exposed under similar conditions, in the same place, with the exception that it would be put into a darkened chamber.

(c) A third portion would be taken to a small four-roomed tenement in Manchester, on a clay soil, without cellarage—and badly ventilated, but it would be placed on the window ledge, with as much light as could there be obtained.

(d) A portion would be placed in the same cottage, but in a dark corner of a sleeping room in which it was known that three deaths from phthisis had occurred within the space of six or seven years.

(e) Finally, a portion would be exposed to used air coming from a ward in a Consumption Hospital, in Bowdon, in darkness. These intentions were fully carried out.

Two collections of sputum were obtained :—

A. From a woman dying of phthisis, collected on April 25. This specimen contained comparatively few bacilli.

B. Also from a woman in an advanced stage of phthisis, collected on April 27. This sputum contained abundance of bacilli.

Sputum (A) was not considered to be very suitable for the purpose owing to the sparseness of bacilli; but it was decided to use it by way of control experiment; owing to an accident, the portions exposed under conditions (c) and (d) were lost.

These collections of sputum were divided into portions and placed in watch glasses marked A. 1, A. 2, A. 3, B. 1, . . . , B 10. Some of these watch glasses were exposed without further arrangement, but others, where there might be a possibility of infection, were enclosed in cages, so arranged that air could reach them through a

* [Since this was written, I have learnt that Savitzky has ascertained that phthisical sputum, exposed "at the ordinary room temperature, and generally under all common life conditions," retains its infectiousness not longer than $2\frac{1}{2}$ months, and, other conditions being equal, a sputum dried in darkness loses its infectious properties within the same period as a sputum exposed to light. 'Med. Chronicle,' Nov. 1890, p. 117.]

thin layer of cotton wool, one kind of cage being constructed of two squares of glass, supported at their edges by cork, and surrounded by cotton wool, the other of small flasks the bottom of which had been cut off, and the lower edge resting in a small circular tray fitted with wool, the mouths of the flasks being also loosely stuffed with the wool.

These watch glasses were then exposed for five weeks under the conditions already noted, commencing on April 29, 1890, with the exception of B. 9 and 10, which were started on May 2. Most of the specimens were withdrawn on June 3; but one, B. 10, was divided on May 13, and a portion, B. 10a, was introduced into a glass bulb and exposed for several minutes each day to a current of ozonised oxygen.

All the specimens were then enclosed in a box and forwarded to the Pathological Laboratory, Owens College, where Dr. Dreschfeld, the Professor of Pathology, had kindly undertaken to carry out the necessary inoculations. Owing to various causes, some of these operations were not commenced until June 27, 1890, others until July 10. The animals used were rabbits, kept under favourable hygienic conditions. The dried sputum was mixed with sterilised water, to form a pasty mass, and this was inserted into the subcutaneous tissue of the back. All the instruments used were made thoroughly aseptic.

The following tables give :—

- (1) The conditions of exposure.
- (2) The date of inoculation.
- (3) The date of death, by killing or otherwise.
- (4) Dr. Dreschfeld's report upon the results of the inoculation.

Table I.—Influence of Dry Soil, Air, and Light.

No. of specimen.	Conditions of exposure.	Date of		Dr. Dreschfeld's reports.
		Inoculation.	Death.	
A. 1	In outdoor studio, Bowdon. In light and free ventilation in flask arrangement	June 27	Killed Sept. 4	Rabbit in good condition; wound completely healed, cicatrix of wound scarcely visible. All inserted sputum completely disappeared, only a few pigmented streaks left, no caseation; internal organs healthy.
A. 2	Ditto, open watch-glass	July 10	Killed Sept. 4	In good condition; wound healed, good cicatrix, no caseous mass. In liver a number of disseminated firm spots; microscopically, these consisted of fibrous tissue; no tubercle bacilli found in them.
B. 6	Ditto in cotton wool cage	July 10	Killed Sept. 4	In good condition; cicatrix healed, no trace of sputum left, no caseation where sputum had been inoculated, only a few pigmented streaks.
B. 7	Ditto in open cage until May 9, then cotton wool added	July 10	Killed Sept. 4	Good condition; cicatrix perfect, some fibrous induration in subcutaneous tissue where sputum had been; no caseation; internal organs healthy.

Table II.—Influence of Dry Soil, Air, and Darkness.

No. of specimen.	Conditions of exposure.	Date of		Dr. Dreschfeld's reports.
		Inoculation.	Death.	
A. 3	In darkened photographic room, Bowdon, in watch-glass	June 27	Killed Sept. 4	Rabbit in good condition, small caseous mass beneath healed wound; all internal organs healthy. Microscopic examination of caseous mass.—Granular detritus, no tubercle bacilli.
B. 8	Ditto in cotton wool cage	June 27	Died Aug. 26	Moderately emaciated; wound healed, but the edges separated on pulling the skin at the sides. In the subcutaneous tissue beneath the wound a few yellowish, soft spots, about the size of pin-heads, surrounded by a zone of hyperæmia. Internally all organs healthy, no signs of tubercle, right heart full of blood, left heart empty. Microscopic examination of the yellow spots shows them to consist of granular detritus and a few granule cells; no tubercle bacilli could be detected.
B. 9	Under ward of Consumptive Hospital in full ventilation, in darkness, in cotton wool cage	July 10	Died Aug. 14	Emaciated; wound healed under a scab, a thin mass of yellow caseous material just beneath the skin. Heart and lungs healthy; kidneys contained a number of small cysts. In the caseous mass a few tubercle bacilli were found.

Table III.—Influence of Clay Soil, Bad Air, and Light.

No. of specimen.	Conditions of exposure.	Date of		Dr. Dreschfeld's reports.
		Inoculation.	Death.	
B. 3	On window-sill of small cottage bedroom in Ancoats. Flask arrangement	June 27	Died Aug. 14	Large rabbit; emaciated. Inoculation wound completely healed, slight scab; no caseous material or any signs of sputum. Internal organs healthy; one white spot found on surface of liver. Microscopic examination of this showed it to consist of round cells, some with one nucleus and others which were polynuclear. At the periphery of the nodule, fibrous tissue. Sections of the nodule showed no tubercle bacilli.
B. 4	Ditto in open watch-glass	July 10	Died Aug. 9	Emaciated, wound at back not healed, and appeared slightly sloughing at the borders. Lungs presented several small caseous nodules; pleura, heart, peritoneum, liver healthy. <i>Tubercle bacilli found in the caseous lung nodules.</i>

Table IV.—Influence of Clay Soil, Bad Air, and Darkness.

No. of specimen.	Conditions of exposure.	Date of		Dr. Dreschfeld's reports.
		Inoculation.	Death.	
B. 1	On dark shelf by fire-place in small cottage bedroom in Ancoats. Flask arrangement	June 27	Killed Sept. 4	Rabbit in good condition; wound completely healed, no caseation, and only a small pigmented spot where the sputum had been deposited; all the internal organs healthy.
B. 5	Ditto in a dark corner near the bed. In watch-glass	July 10	Killed Sept. 4	Rabbit in good condition; cicatrix where the wound was, and beneath it a caseous mass about the size of a bean. Examined microscopically, this mass contained <i>tubercle bacilli</i> . Nothing abnormal in any of the organs.

Table V.—Influence of Dry Soil, Bad Air, and Darkness

No. of specimen.	Conditions of exposure.	Date of		Dr. Dreschfeld's reports.
		Inoculation.	Death.	
B. 10	Cotton wool cage, in ventilating shaft from ward of Consumption Hospital for ten days, then placed on top of bookcase in sitting-room	July 10	Died Aug. 14	Emaciated; wound healed under a scab. A yellow caseous mass about the size of a small pea beneath the scab. The liver presented a few yellowish nodules; all the other organs sound. In the caseous mass a few <i>tubercle bacilli</i> were found; none in the liver.
B. 10	A portion of the above was taken on the tenth day and exposed to a current of ozonised oxygen for a few minutes daily for a fortnight	July 10	Killed Sept. 4	Rabbit fairly well nourished; cicatrix quite healed, no trace of inoculated matter, and no trace of caseation. In the left lung one firm nodule; this was carefully examined microscopically and showed no bacilli. It was apparently only thickened pleura.

It will be seen that none of the four specimens of sputum exposed to fresh air and light on a dry soil conveyed the disease, but one of the three portions exposed under similar conditions in darkness produced tubercle.

Of the two exposed in the cottage in Ancoats in the light one produced tubercle, and of the two specimens exposed in the same place, in comparative darkness, one caused tubercle, the other did not.

Lastly, the specimen placed in the ventilating shaft from a ward in the Consumption Hospital, Bowdon, on a dry soil, conveyed the disease, and the portion removed from it after ten days and exposed to the action of ozonised oxygen did not produce tubercle.

These experiments are too few in number to justify the statement of positive conclusions, but, so far as they extend, they go to prove that fresh air and light and a dry sandy soil have a distinct influence in arresting the virulence of the tubercle bacillus; that darkness somewhat interferes with this disinfectant action; but that the mere exposure to light in otherwise bad sanitary conditions does not destroy the virus. There are also some indications that the presence of a cotton wool envelope may interfere somewhat with the action for good or evil of both good and bad air respectively.

Further observations are now being made with sputum exposed by Professor Tyndall at Bel Alp, Switzerland, in light and darkness, each kind for 10 days and 14 days respectively, and compared with the same sputum exposed in the same cottage in Ancoats.

The pathological results of these specimens have not yet been made out. The results will be given in a future note.

The Society adjourned over the Christmas Recess to Thursday, January 8th, 1891.

Presents, December 18, 1890.

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January 8, 1891.

Lieut.-General STRACHEY, R.E., Vice-President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

- I. "On the Minute Structure of the Muscle-columns or Sarcostyles which form the Wing Muscles of Insects. Preliminary Note." By E. A. SCHÄFER, F.R.S. Received December 15, 1890.

[Publication deferred.]

- II. "On the Minute Structure of Striped Muscle, with Special Reference to a New Method of Investigation, by means of 'Impressions' stamped in Collodion." By JOHN BERRY HAYCRAFT, M.D., D.Sc., F.R.S.E. Communicated by Dr. KLEIN, F.R.S. (From the Physiological Laboratory, Univ. Edin.) Received January 2, 1891.

[Publication deferred.]

- III. "On the Reflection and Refraction of Light at the Surface of a Magnetised Medium." By A. B. BASSET, M.A., F.R.S. Received December 17, 1890.

(Abstract.)

The object of the present paper is to endeavour to ascertain how far the electromagnetic theory of light, as at present developed, is capable of giving a theoretical explanation of Dr. Kerr's experiments* on the effect of magnetism on light.

In the first series of experiments, polarised light was reflected from the polished pole of an electromagnet, so that the lines of magnetic force were perpendicular to the reflecting surface; and in the second

* 'Phil. Mag.,' May, 1877, and March, 1878.

series, the light was reflected from a polished plate of soft iron laid upon the poles of a horseshoe electromagnet, so that the lines of magnetic force were parallel to the reflecting surface. In both series of experiments, it was found that, when the circuit was closed, so that the reflector became magnetised, the reflected light exhibited certain peculiarities, which disappeared when the current was off. It was also found that the effects of magnetisation were, in most cases, reversed when the direction of the magnetising current was reversed; that is to say, if the intensity of the reflected light was strengthened by a right-handed current, it was weakened by a left-handed one.

In these experiments, a metallic reflector was employed, and consequently the results were complicated by the influence of metallic reflection; it therefore seems hopeless to attempt to give a complete theoretical explanation of these experiments until a satisfactory electromagnetic theory of metallic reflection has been discovered; and this, I believe, has not yet been done.

There are, however, several non-metallic substances (such as strong solutions of certain chemical compounds of iron), which are capable, when magnetised, of producing an effect upon light; and the theoretical explanation of the magnetic action of such substances upon light is accordingly free from the difficulties surrounding metallic reflection. I have accordingly, in the present paper, attempted to develop a theory which is applicable to such media.

The theory, which is due to Professor Rowland, is founded upon the following considerations:—

It was proved by Hall* that, when a current passes through a conductor which is placed in a strong magnetic field, an electromotive force is produced, whose intensity is proportional to the product of the current and the magnetic force, and whose direction is at right angles to the plane containing the current and the magnetic force. Professor Rowland† has assumed that this result holds good in a dielectric which is under the action of a strong magnetic force; accordingly the general equations of electromotive force become

$$P = -\frac{dF}{dt} - C(\gamma g - \beta h) - \frac{d\psi}{dx} \dots\dots\dots (1),$$

where α, β, γ are the components of the external magnetic force, and C is a constant which depends upon the physical constitution of the medium.

Writing $p_1 = C\alpha$, &c., it follows that, if the medium is isotropic, the equations of electric displacement are of the form

* 'Phil. Mag.,' March, 1880.

† *Ibid.*, April, 1881.

$$\frac{d^2 f}{dt^2} = \frac{1}{\mu K} \nabla^2 f + \frac{1}{4\pi\mu} \left(p_1 \frac{d}{dx} + p_2 \frac{d}{dy} + p_3 \frac{d}{dz} \right) \left(\frac{d\dot{g}}{dz} - \frac{d\dot{h}}{dy} \right) \dots (2).$$

The boundary conditions are : continuity of magnetic induction and electric displacement perpendicular to the reflecting surface, the latter of which is equivalent to continuity of magnetic force perpendicular to the plane of incidence ; continuity of magnetic force along the line of intersection of the plane of incidence with the reflecting surface ; continuity of the rate at which energy flows across the reflecting surface. Now the refracted light consists of two waves, circularly polarised in opposite directions, and the reflected light is elliptically polarised ; we have, therefore, four equations to determine the amplitudes of the two refracted waves, and the amplitudes of the two components of the reflected wave.

The results of the paper agree with Dr. Kerr's experiments in the following particulars :—

- (i.) The reflected light is elliptically polarised.
- (ii.) When the magnetisation is parallel to the reflecting surface, no effect is produced when the incidence is normal, or when the plane of incidence is perpendicular to the direction of magnetisation.
- (iii.) When the plane of incidence is parallel to the direction of magnetisation, and the light is polarised *in* the plane of incidence, the magnetic term increases from grazing incidence to a maximum value, and then decreases to normal incidence.

The principal point of disagreement is, that in all cases the intensity of the reflected light is unchanged when the direction of the magnetising current is reversed.

I do not think that the results of the theory can be considered altogether unsatisfactory, since they certainly explain some of Dr. Kerr's experimental results ; and I am disposed to think that the disagreement is due to the disturbing influence of metallic reflection. At the same time, the question is one which can only be decided by experiment, and it is therefore most desirable that experiments on magnetised solutions should be made.

IV. "Further Contributions to the Metallurgy of Bismuth."

By EDWARD MATTHEY, F.S.A., F.C.S., Assoc. Roy. Sch. Mines. Communicated by Sir G. G. STOKES, Bart., F.R.S.
Received December 22, 1890.

In October, 1887, I read a paper before the Royal Society* upon a new method which I incidentally discovered while working with a view to separate copper from bismuth, by fusion with bismuth sulphide,

* 'Roy. Soc. Proc.,' vol. 43, p. 172.

I stated in this paper that bismuth "frequently contains a small proportion of copper, an element most detrimental even in small traces, and hitherto only eliminated by a wet process, costly in practice and tedious in operation. It is necessary by such method to dissolve up the whole of the alloy, and precipitate the bismuth in the usual manner—a bulky operation, and one requiring a considerable amount of time. It became therefore advisable, in order to treat cupriferous bismuth rapidly and upon a commercial scale, to effect this separation, if possible, by means of a dry process."

In further researches in the metallurgy of this interesting metal, a case was found in which bismuth contained a very small proportion of copper, under 0.5 per cent., but sufficient to render the metal useless, and in fact, to destroy those characteristic properties upon which its industrial applications depend.

Instead of treating this cupriferous bismuth by fusion with bismuth sulphide, which necessitates a temperature sufficiently elevated to bring about a complete fusion of the bismuth sulphide, and consequently, unless very great care be taken, great loss by volatilisation of bismuth, it occurred to me to fuse the alloy, and, at a temperature a little above its melting point, to add a small proportion of sodium monosulphide. The mass was then stirred well, so as to bring every portion of the fused alloy into contact with the fused sulphide.

After about one hour's stirring, a test was made of the molten metal, and it was found that the amount of copper in it was very considerably decreased.

By skimming off the film of scoria which had risen to the surface, adding a further small proportion of the sodium monosulphide, and continuing the operation of stirring, every trace of copper was eliminated, and the bismuth so freed from copper rendered in every way suitable for industrial use.

The first experiment was made upon a quantity of 105 kilograms, which yielded 94 kilograms of bismuth free from copper, and about 11 kilograms of skimmings containing the whole of the copper, their bismuth contents of course being available for reduction with further and larger quantities of skimmings as they accumulated.

This process has been repeated upon very considerable quantities of cupriferous bismuth, and has proved to be successful.

This question of keeping the temperature low is of much importance, for the lower the temperature the less tendency there is for the bismuth to volatilise, and as it is necessary to obtain the bismuth free from traces of impurity, which entirely change its nature, it will be seen that any improvement in manipulation, or in the process itself, which enables pure metal to be obtained possesses much interest.

Presents, January 8, 1891.

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Mr. G. J. Symons, F.R.S.

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Mezzotint Engraving of Sir W. Bowman, Bart., F.R.S., from the painting by W. W. Ouless, R.A., exhibited at the Royal Academy, 1889.

The Committee of Subscribers.

January 15, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

- I. "On the Rate of Propagation of the Luminous Discharge of Electricity through a Rarefied Gas." By J. J. THOMSON, M.A., F.R.S., Cavendish Professor of Experimental Physics, Cambridge. Received January 2, 1891.

Though the determination of the velocity of propagation of the luminosity which accompanies the electric discharge through gases might well be expected to throw considerable light on the means by which the discharge is effected, as far as I can find, no attempts seem to have been made in this direction since Wheatstone, in 1835, observed the appearance presented in a rotating mirror of the discharge through a vacuum tube 6 feet long; he concluded from his observations that the velocity with which the flash went through the tube could not have been less than 2×10^7 cm. per second. This very great velocity does not seem to be accompanied by a correspondingly large velocity of the luminous molecules, for von Jahn (Wiedemann's 'Annalen,' vol. 8, 1879, p. 675) has shown that the lines of the spectrum of the gas in the discharge tube are not displaced by as much as 1/40 of the distance between the D lines when

the line of sight is in the direction of the discharge tube. It follows from this, by Döppler's principle, that the particles when emitting light are not travelling in the direction of the discharge at the rate of more than a mile a second, proving at any rate that the luminosity does not consist of a wind of luminous particles travelling with the velocity of the discharge.

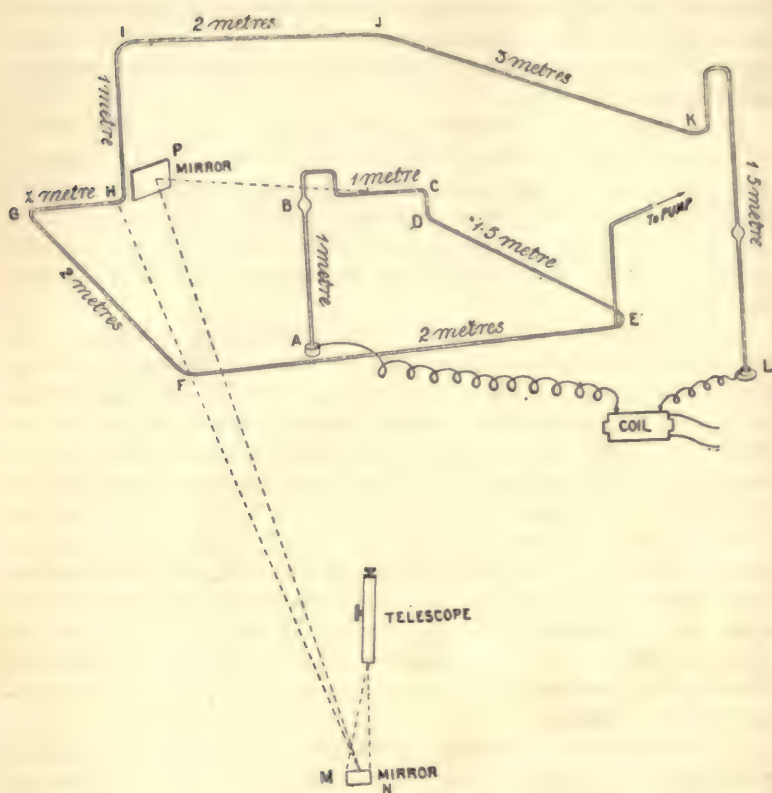
Wheatstone's observations only give an inferior limit to the velocity of the discharge. Nothing was observed in these which indicated that the velocity of discharge was finite. A method which would enable us to measure this velocity would also at the same time show whether the discharge always started from the positive or negative end of the tube, and so enable us to trace the course of the discharge.

In the following experiments I have endeavoured to measure this velocity, and also to ascertain whether the main discharge starts from the anode or the cathode. The long tubes used in my experiments were practically filled by the positive column; thus in the tube 50 feet long the positive column extends to within an inch or two of the cathode. All the experiments described below relate to the behaviour of the positive column.

Plücker (Poggendorff's 'Annalen,' vol. 107, 1859, p. 89) concludes from the action of a magnet on the discharge that it starts from the anode. This conclusion does not seem to have met with much acceptance; my experiments, however, fully bear it out, as I find that, except under exceptional circumstances, which will be described later, the luminosity of the positive column begins close to the anode and travels away from it.

The experiments for measuring the velocity of the luminous column were after several preliminary trials finally arranged in the following way. ABCDEFG . . . L (fig. 1) is a glass tube about 15 metres long and 5 mm. in diameter, which, with the exception of two horizontal pieces of BC and GH, is covered with lamp black; this tube is exhausted, and a current sent through it from a coil giving sparks 6 or 7 inches long in air; the light from the uncovered portions falls on a rotating mirror MN, placed at a distance of about 6 metres from BC; the light from GH falls on the rotating mirror directly, that from BC after reflection from the plain mirror P. The images of the bright portions of the tube after reflection from the revolving mirror are viewed through a telescope, and the mirrors are so arranged that when the revolving mirror is stationary the images of the bright portions of the tube appear as portions of the same horizontal straight line. The terminals of the long vacuum tube are pushed through mercury up the vertical tubes AB, KL. This arrangement was adopted because by running sulphuric acid up these tubes the terminals could readily be changed from pointed platinum wires to flat liquid sur-

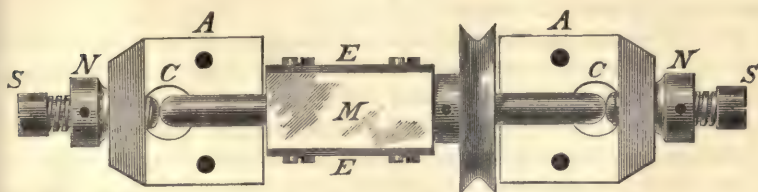
FIG. 1.



faces, and the effect of very different terminals on the velocity and direction of the discharge readily investigated. The bulbs in these vertical tubes were also found very useful as receptacles for sulphuric acid for drying the gas left in the tube.

The revolving mirror was driven at a speed varying from 400 to 500 per second by a Gramme machine through which the current from twelve large storage cells was sent. The mirror first used was mounted on ball bearings such as are used for bicycles; the axle had, however, too much play, and it was eventually discarded for one made by the Cambridge Scientific Instrument Company, and designed by Mr. Bartlett, the assistant at the Cavendish Laboratory. It is represented full size in fig. 2. The spindle carrying the mirror M runs on parallel bearings in the uprights A, A. The ends of the spindle are rounded, hardened, and polished, and are in line with but just do not

FIG. 2.



Plan of Revolving Mirror. Upper halves of Bearings, A, carrying Oil Cup removed.

touch two set screws, S, S, whose ends are also rounded, hardened, and polished. Directly beneath the end of each axle is a cavity, C, which serves to hold the oil running down from oil cups placed immediately above. This arrangement was found to lubricate so well that there was no appreciable heating even after long runs. The spindle is made from square steel, the ends being turned down, and against two opposite sides of the square centre portion, clutches E, E, for holding the mirror M, are fastened. The whole is accurately balanced. The bearings are attached to a heavy iron casting, which is firmly bolted down to a heavy piece of masonry.

In order to get a sufficiently rapid rotation of the revolving mirror the Gramme had to be geared up. This was done by means of pulleys mounted on ball bearings.

A great many arrangements were tried in order to break the primary circuit of the coil, when the mirror was in such a position that the images of the luminous part of the tube would be reflected by it into the telescope; after a great deal of time had been spent over these they were all given up. The reason why they will not work is pretty clear. The coil will not work when the primary circuit is broken anything like so often as 500 times a second, so that if the primary is to be broken by the mirror there must be very considerable gearing down between the mirror and the break, in other words, the mirror can be moved through a very considerable angle without moving the break through more than a very small distance, but almost the smallest possible movement of the mirror is sufficient to send the images out of the field of view; and it was found impossible to diminish the play between the mirror and the break to such an extent as to ensure that at a high rate of rotation the break took place synchronously with the requisite position of the mirror. The method finally adopted was the primitive one of using an independent mercury break driven by a small Thirlmere water motor, and patiently looking through the telescope until the break happened to occur just at the right moment. This, though a somewhat lengthy proceeding, was not found in practice to be any longer than when synchronism be-

tween the break of the coil and the position of the mirror was attempted by artificial means.

When the observations were made in this way, the observer at the telescope saw, on an average about once in four minutes, sharp bright images of the portions BC and GH of the tube, not sensibly broadened, but no longer quite in the same straight line; the relative displacement of these lines was reversed on reversing the coil, and also on reversing the direction of rotation of the mirror. These bright images are not the only ones observed through the telescope; ill-defined and widened images were much more frequent; sometimes these were widened out so as to fill the whole field of view with a luminous haze, at others, the images appeared as broad bands, the boundaries of these bands not being in the same straight line; these images indicate a discharge lasting for a very much longer time than that which produced the bright sharp images which were the object of our attention. When these sharp images were very bright, it could be distinctly seen that they were striated.

The displacement of the images from the same straight line is due to the finite velocity with which the luminosity is propagated; for if the mirror can turn through an appreciable angle while the luminosity travels from BC to GH, or *vice versâ*, these images of BC and GH, as seen in the telescope after reflection from the revolving mirror, will no longer be in the same straight line, but if the mirror is turning so that, on looking through the telescope, the images seem to come in at the top and go out at the bottom of the field of view, the image of that part of the tube at which the luminosity appears first will be raised above that of the other. If we know the rate of rotation of the mirror, the vertical displacement of the images and the distance between BC and GH, the rate of propagation of the luminosity may be calculated. The observations were made in the following way:—The tube having previously been properly dried and exhausted, so that the discharge would pass freely through it, one observer took his seat at the telescope, the room was then darkened, and the coil set in action by another observer, the observer at the telescope not knowing which of the electrodes was positive and which negative; the mirror was then set in rotation, and about once in four minutes, on an average, the observer at the telescope saw the bright sharp images alluded to above. When the observer had seen two or three of these, he stated whether they were very bright, fairly bright, or indistinct; which image was the higher, and by how much. The distance between the images was estimated in terms of the apparent distance between the divisions of a vertical millimetre scale placed at GH when seen through the telescope; the scale was not seen at the same time as the image of the tube, so that the observation cannot claim any great accuracy; different observers agree, however, under the same circum-

stances to within 25 per cent, and it is probable that, in our present state of knowledge about the discharge of electricity through gases, the points of most importance can be settled by a somewhat rough determination of the velocity of propagation.

Some hundreds of observations were made, and in every case in which the observer declared the images to be very bright, and in every case but one in which the images were declared to be fairly bright, the displacements (if there was not a *very* large air break in the circuit) corresponded to the luminosity travelling from the positive to the negative electrode. When AB was the negative electrode, the luminous discharge arrived at GH, a place about 25 feet from the positive electrode, before it reached BC, which is only a few inches from the cathode, and as the interval between its appearance at these places was about the same as when the current was reversed, we may conclude that, when AB is the cathode, the luminosity, which is found only a few inches from it at BC, has started from the positive electrode, and traversed a path enormously longer than its distance from the cathode. We thus arrive at the conclusion that the positive column, which in a long tube like the one under consideration practically fills the tube, since it extends to within an inch or two of the anode, starts from the positive electrode.

In view of the probability that the passage of the current from the electrodes to the gas might be largely influenced by chemical action between the electrode and the gas, I repeated the experiments with electrodes of very different kinds; the result, however, was the same, whether the electrodes were pointed platinum wires, carbon filaments, flat surfaces of sulphuric acid, or the one electrode a flat liquid surface and the other a sharp pointed wire. The positive column starts from the positive electrode, even though this is a flat liquid surface while the negative is a sharp-pointed wire.

Velocity of Propagation of the Discharge.

The displacement of the images of the two luminous portions of the tube caused by the rotation of the mirror was equal to the distance between the images of divisions 1.5 mm. apart on a vertical scale placed at GH. Thus, if T is the time the luminosity takes to travel from BC to GH, since the distance of the mirror from the luminous part of the tube is 6 metres, the circular measure of the angle turned through in the time T is $1.5/12,000$. If n is the number of revolutions made by the mirror per second,

$$T = \frac{1.5}{12,000 \times 2\pi n}.$$

When the mirror was running at full speed, its rate was pretty constant and, as determined by the note given out in a telephone (taken for the sake of avoiding the noise made by the Gramme and mirror to an adjoining room) when the circuit was broken once in each revolution of the mirror, and also by the velocity of the band driving the mirror, was about 480 per second. The distance between the places BC and GH is 7 metres, so that if v is the rate at which the luminosity of the positive column travels,

$$T = \frac{700}{v};$$

$$\begin{aligned} \text{hence} \quad v &= 12000 \times 2\pi \times 480 \times 700 \times \frac{1}{1.5} \\ &= 1.6 \times 10^{10}, \end{aligned}$$

or rather more than half the velocity of light; but, as I explained before, this must be regarded as an approximation, rather than as an accurate determination. It is sufficient, however, to show that the luminosity of the positive column travels through a vacuum tube with a velocity comparable to that of light.

The preceding results hold when there is a short air break in the circuit, but, if the air break is increased until the coil can only spark through the tube with difficulty, the luminosity seems inclined to start from the air-break electrode, and the direction in which it travels is not always reversed by reversing the coil.

The fact that the main portion of the luminous discharge in a long vacuum tube has its origin at the positive electrode may appear at first sight inconsistent with the result that glow discharge takes place more easily, that is, with a smaller value of the electromotive intensity, at the negative than at the positive electrode. Thus Faraday states that the discharge from a sphere takes place more easily when the sphere is negatively than when it is positively electrified.

Again, ultra-violet light can produce a discharge from a negatively but not from a positively electrified piece of metal. Thus Lenard and Wolf (Wiedemann's '*Annalen*,' vol. 37, 1889, p. 443) have proved that we can produce a cathode by allowing ultra-violet light to fall on a negatively electrified plate, while no discharge occurs if the plate is positively electrified, and Hallwachs (Wiedemann's '*Annalen*,' vol. 34, 1888, p. 731) and Righi have shown that when ultra-violet light falls on an unelectrified piece of metal the metal becomes positively charged, *i.e.*, the light converts it into a cathode.

These considerations do not, however, seem to affect the question we are considering when the electromotive force is sufficiently great

to produce a discharge from the positive electrode; a much more important consideration in this case is the relative time required by the two electricities to leave their respective electrodes. If the time taken by the positive electricity to leave the anode is very much less than that taken by the negative to leave the cathode, and especially if this second time is greater than the time taken by the luminosity to pass over a considerable length of the tube, there could be no difficulty in understanding how the luminosity of the positive column, which in these experiments practically fills the tube, should have its origin at the anode.

Now, Spottiswoode and Moulton, in their very remarkable paper on the "Sensitive State of the Electric Discharge" ('Phil. Trans.,' 1879, p. 165), investigated the relative magnitudes of the times occupied by the various processes which go to make up the electric discharge, and by means of the phenomena which are observed in the revocation of what are called by them relief effects, show (1) that the time taken by the negative electricity to leave the cathode is so much longer than the time taken by the positive electricity to leave the anode, that the two times may be considered to belong to different orders of small quantities; and (2) that the time taken by the negative electricity to leave the cathode is greater than the time taken by the luminosity to travel over the length of the tube (in their case the tube was not very long); remembering these facts, the result which we have obtained by the use of the revolving mirror need occasion us no surprise.

These experiments lead us to regard the discharge as the sweeping down of the positive electricity from the anode with an enormous velocity (about half that of light in our experiments), accompanied by what is comparatively a very slow discharge from the cathode.

The fact that the positive electricity leaves the anode more quickly than the negative does the cathode, explains a very prominent feature of the electric discharge: the accumulation of positive electricity in the neighbourhood of the cathode. The positive electricity arrives at the region surrounding the cathode before the discharge from this terminal is completed; thus there will, during the greater part of the discharge from the cathode, be an excess of free positive electricity in the neighbourhood of the electrode, and, if the discharges succeed each other with sufficient rapidity, the positive electricity will accumulate until the effect of its attraction is sufficiently great to cause the negative electricity to leave the cathode as fast as the positive electricity arrives.

The explanation of the exceedingly rapid rate of propagation of the positive column is of primary importance in any theory of the mechanism of the electric discharge. The theory which seems to me the most probable is that the passage of electricity (or, from another

point of view, the shortening of tubes of electrostatic induction) is effected by the dissociation of the molecules into atoms, in other words, that "chemical decomposition is not to be considered as an accidental attendant on the electrical discharge, but as an essential feature of the discharge, without which it could not occur" ('Phil. Mag.,' vol. 15, 1883, p. 432). Free atoms must, on this view, exist in the path of the discharge to serve as the ends of the tubes of force as they shorten. If, however, we take this view of the discharge of electricity, the chemical decomposition attendant on the discharge along the positive column cannot consist of the *consecutive* interchange of atoms between adjacent molecules, for, since on this view each atom would have to move up to the one in the adjacent molecule, the velocity of the atoms would have to be that of the discharge of the positive column, viz., about half that of light. The existence of a wind in the tube of this velocity is, *a priori*, unlikely, and the following calculation will show that it would require the expenditure of more energy than we have at our disposal.

Let us take the case of the discharge of a parallel plate condenser, the distance between the plates being 1 cm. Let F be the electromotive intensity between the plates, K the specific inductive capacity of the gas; then the energy per square centimetre of area of the condenser plate is

$$\frac{1}{8\pi} KF^2.$$

Let N_0 be the number of atoms required to discharge unit area of the condenser; then, if σ is the density of the electricity on the condenser and ϵ the charge on each atom in electro-magnetic measure,

$$N_0\epsilon = \sigma.$$

If m is the mass of one of these atoms, v the velocity with which the atoms move, their kinetic energy is

$$\frac{1}{2}N_0mv^2.$$

If N is the number of atoms in one gramme of the substance, then, if the charges on the atoms are the same as that deduced from electrolytic considerations,

$$N\epsilon = 10^4 \text{ and } Nm = 1.$$

Now

$$4\pi\sigma = KF.$$

Making these substitutions, we find that the kinetic energy of the atoms is

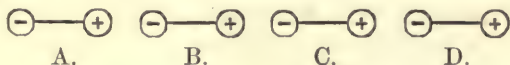
$$\frac{1}{8\pi} \frac{KFv^2}{10^4},$$

so that the ratio of the kinetic energy of the atoms to the energy in the electric field is

$$\frac{v^2}{10^4 \cdot F}$$

Now, at atmospheric pressure, F for air is about 3×10^{12} ; we have seen that $v = 1.6 \times 10^{10}$; hence, in this case, the kinetic energy of the atoms is about 8000 times that of the electric field. If we had taken the case of a gas at a lower pressure, the disproportion would have been still greater.

For this reason, the discharge along the positive column cannot be carried by atoms travelling at the same rate as the discharge; the same argument would also be fatal to the view that the discharge takes place by the consecutive interchange of atoms between adjacent molecules. If, therefore, we are to retain the view (which seems to me to be almost established by the results of recent experiments) that the passage of electricity is effected by the dissociation of the molecules in the path of the discharge, we are precluded from supposing that in the positive column the discharge takes place by the molecules dissociating one after another, as the discharge comes up to them. In a paper in the 'Philosophical Magazine' for August, 1890, I suggested that we could reconcile the dissociation theory with the observed velocity of propagation of the discharge (of which I had, at that time, only obtained an inferior limit, and did not know that it started from the anode), by supposing that the molecules of the dielectric in the path of the discharge, before the discharge takes place, form themselves into a series of Grotthus chains, and that for the molecules which constitute any one of these chains, the dissociation and recombination go on simultaneously. This may, perhaps, be made clearer by a somewhat crude illustration.



If A, B, C, D represent consecutive polarised molecules, the simplest view of the discharge would be to suppose A to split up into atoms, its positive atom going up to B and combining with the negative atom of that molecule; the positive atom of B is driven off, and travels to C and combines with the negative atom, and so on. On this view the velocity of the atoms would be very nearly that of the discharge which other preceding experiments have shown to be inadmissible for the positive column. If, however, we suppose that the molecules A, B, C, D, constituting a Grotthus chain, are split up simultaneously, and that while the positive atom of A combines with the negative of B, the positive atom of B is combining with the negative of C, and so on, then, in the time which elapses between

the commencement of the dissociation of a molecule to the end of the recombination of its atom with those of neighbouring molecules, a positive atom will have disappeared from one end of the chain and appeared at the other. Thus in this case, since the time taken for the decomposition and recombination of the molecules is independent of the length of the chain, whatever the length of the chain may be, the positive charge will travel from one end of the chain to the other in the same time, and thus the velocity of the discharge will be proportional to the length of the chain. In the paper referred to above, it is suggested that the high velocity of the discharge of the positive column is attained by the formation of Grotthus chains of suitable length, the column thus consisting of a series of separate discharges, the length of each discharge being that of the Grotthus chain; these separate discharges are made manifest in the stratification which is so striking a feature of the positive column, the space between the bright portions of two striæ corresponding to the length of the Grotthus chain; thus, on this view, the stratifications are the manifestations of the machinery which enable the positive discharge to travel at such a rate. In the paper in the '*Philosophical Magazine*' it is shown that this view of the discharge agrees well with what is known as to the behaviour of striæ.

The preceding experiments show that the tubes of force which we imagine as stretching round the circuit, and contracting when the discharge takes place, are anchored almost completely to the negative electrode. When the discharge begins to pass, the ends of these tubes near the positive electrode will be agitated in an approximately periodic way, electrical vibrations will travel along the tubes with the velocity of light, and, as one end of the tube is fixed, these will form stationary vibrations; these stationary vibrations may be conceived to give the molecules of the gas in the tube a certain periodicity of arrangement, and lead to the formation of the Grotthus chains of definite lengths, required by the preceding explanation. It will be seen that this would make the position of the striæ depend on that of the negative electrode, so that when the latter is moved the striæ ought to be displaced; this effect has been observed by Goldstein.

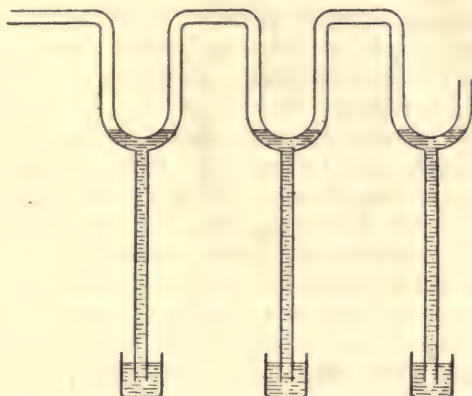
As an alternative to the preceding view, it might perhaps be urged that the luminosity of the positive column outruns the positive discharge. This view, however, seems to be quite untenable in the face of Spottiswoode's and Moulton's experiments on the sensitive state of the electric discharge ('*Phil. Trans.*'), for the relief effects observed in their experiments seem to show, without ambiguity, that the positive luminosity is coincident with free positive electricity.

We have also no evidence that a gas can be made luminous by

sudden alterations in the electric or magnetic intensity of the field in which it is placed, unless these are accompanied by the passage of free electricity through the gas.

In order to get some further information about the laws which govern the propagation of the positive column, some experiments were made in which the discharge had to pass from the gas to mercury and out again from the mercury to the glass several times in its passage from B to G (fig. 1). The arrangement by which this was done is shown in fig. 3. Pieces of glass tubing, bent as in the figure, with baro-

FIG. 3.



metric tubes filled with mercury attached to their lowest points, were inserted in the circuit between B and G. By raising or lowering the vessels into which the ends of the barometer tubes dipped, mercury could be poured into or taken out of the bends in the tube. There were in all six of these mercury electrodes introduced between B and G. The displacement of the images, as seen through the telescopes, was observed (1) when the mercury was below the level of the tops of the barometer tubes, and (2) when the mercury filled the bends of the tube, blocking it up completely in six places. No appreciable difference could be observed between the displacements of the images in the two cases. When, however, the mercury was in the tube, the discharge had very much greater difficulty in getting through than when its path was not interrupted by the columns of mercury; this was shown by the luminosity in the main circuit being very much fainter, and that in a branch circuit leading to the air-pump much brighter, when the mercury was in the tubes than when it was not.

It seems, I think, pretty clear that what takes place when the

mercury is in the tube is something of the following kind. The positive electricity rushes from the anode down the tube until it reaches the first mercury plug; it attracts the negative electricity to the nearer end of this plug, and repels the positive to the other end; this positive electricity begins to leave the mercury immediately and travels down to the next mercury plug.

The positive electricity which travels up to the first mercury plug and the negative electrification it produces on the mercury form an electrical double layer which takes some time to disappear, longer probably than the time taken by the electricity to travel from one end of the tube to the other. The time the luminosity takes to travel from B to G will thus not be much affected by the mercury plugs; but, as the discharge leaves behind it a series of electrical double layers on the sides of the mercury columns nearest the positive electrode, the difficulty of forcing electricity through the tube will be temporarily increased.

It is, I think, worthy of remark that the effects produced by displacement currents render it impossible to predict the velocity of the discharge of electricity through a rarefied gas. For, if we consider the processes which accompany this discharge, we have, first, the production of the electric field; this causes an increase in the electric displacement, and in consequence produces magnetic effects; and the displacement current behaves as if it had inertia, travelling through the medium with the velocity of light. When the intensity of the field is increased sufficiently to cause discharge, the electricity passes through the gas, and the electric field disappears. The convective current formed by the passage of the free electricity is balanced by the displacement current in the opposite direction, due to the disappearance of the electric displacement. The discharge, therefore, does not produce a magnetic field, and has, therefore, no inertia. The velocity of propagation of this discharge will, therefore, be governed by different laws from those which control currents producing a magnetic field, and need not, therefore, have anything to do with the velocity of propagation of light through the medium.

By adjusting the circumstances under which the preliminary charging takes place, we can separate the magnetic force due to the charging by as long an interval as we please from the discharge. We can also, by charging sufficiently slowly, make the magnetic force at any instant as small as we please; thus it is conceivable that we might have a copious discharge of electricity through a gas practically unaccompanied by magnetic force.

The very remarkable action of a magnet on the electric discharge is not inconsistent with this view, as on it the discharge consists of two equal and opposite currents, of which only one is

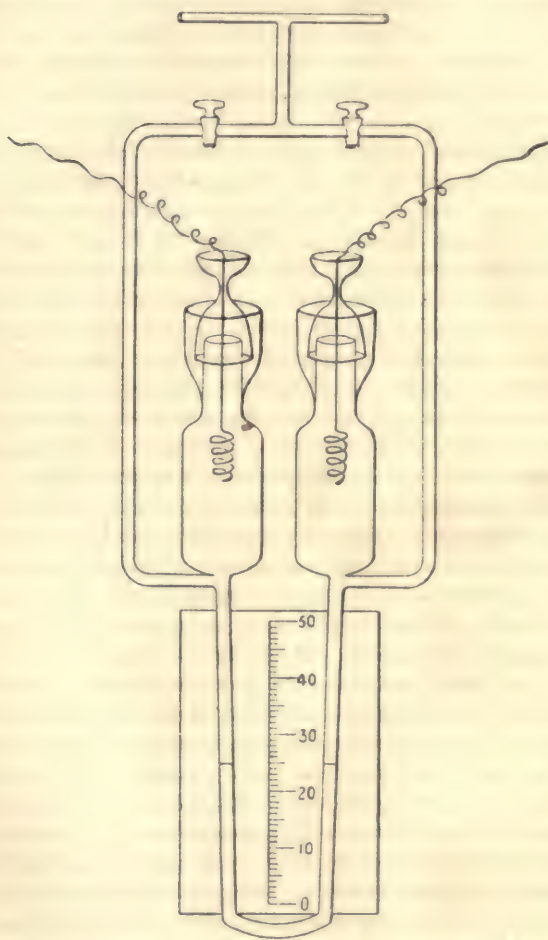
visible: we see the action on the visible current, but not the opposite one on the other.

The most obvious explanation of the remarkable difference in the behaviour of the discharges from the anode and cathode is that it arises from some difference in the chemical action between the gas and the two electrodes. I have made a series of experiments in order to test this view, and have been led to the conclusion that an explanation of this effect by purely chemical action is delusive. At the same time I think that the necessity for the existence of some action between the gas and the electrode is shown by the following experiment. In the 'Philosophical Magazine,' vol. 29, 1890, p. 441 (On the Passage of Electricity through Hot Gases), I described an experiment in which cold electrodes were plunged into a hot gas, such as iodine, heated until it dissociated, when it was found that no current passed through the gas until the electrode got hot, when it passed freely. The effect produced by the cold electrodes seemed too abrupt to be altogether due to the cooling of the adjacent gas by the electrodes. I therefore made the following experiment in order to test this point. If the effect is due to the cooling of the gas, the temperature of the electrodes when the system begins to conduct ought not to vary much, whatever may be the material of which they are made; while if the effect were due to chemical action between the gas and the electrodes, we should expect considerable variation with different electrodes in the temperature at which conduction begins. I therefore attempted to measure roughly the temperature at which conduction began (*a*) when the electrodes were iron, (*b*) when they were platinum. This was done by making one of the electrodes into a thermopile placed in circuit with a dead-beat galvanometer; in case (*a*) the thermopile consisted of an iron plate with a German-silver wire, in case (*b*) of platinum foil and a German-silver wire. The electrode used as the thermopile was dropped cold into the hot gases and connected up with the main circuit. When the galvanometer in the main circuit first began to show decided indications of the passage of a current, the observer who was watching this galvanometer called to the observer at the dead-beat galvanometer in the thermopile circuit, and this observer read the deflection of this galvanometer. From this reading the temperature of the hot junction could approximately be determined. The experiment was repeated, using, instead of the iron-German silver couple, a platinum-German one, the platinum foil being wound round an iron plate to make it heat up at approximately the same rate as the first couple. It was found that the conduction began at a much lower temperature when the electrode was iron than when it was platinum, indicating that some action between the electricity and the gas was necessary for conduction. I could not,

however, detect any difference between the positive and negative electrodes in this respect. The gases in which this effect was found were I, HCl, and HI.

I next endeavoured to see if I could detect any difference in the chemical action of chlorine on a metal positively or negatively electrified. This was done in the following way:—A and B (fig. 4) are

FIG. 4.



two coils of copper wire, of the same length, made from the same hank of wire, and as nearly as possible alike in all respects: these are filled into two equal vessels which are connected by a U-tube

filled with sulphuric acid, which serves to indicate any difference in the absorption of the chlorine by the two coils of wire. The vessel was exhausted and then filled with chlorine, and A and B were placed in parallel with the electrodes of an induction coil, giving sparks about an inch and a half long. In this way one coil was positively and the other negatively electrified, and any difference in the rate of combination of the chlorine with the metal would show itself by the motion of the sulphuric acid in the gauge. Only a very small motion of the sulphuric acid occurred, and this seemed to be accidental, as it was not reversed on reversing the coil. The difference between the rate of combination of chlorine with a positively and negatively electrified metal must therefore be small.

Again, if the difference between the behaviour of the positive and negative discharge were due to purely chemical action between the gas and the electrode, we should expect this difference to be absent in the case where the electrodes consisted of a volatile liquid or solid, and the gas was the vapour of the electrode. I tried three cases of this kind: one in which the electrodes were water and the gas water vapour; a glass tube was completely filled with water, then placed on the pump, and the water boiled away until only just enough was left to serve as electrodes; the tube was then sealed off and cooled down until the vapour pressure was low enough to allow the electric discharge to pass without difficulty; this tube, however, had all the usual characteristics of the discharge through vacuum tubes, including the negative dark spaces and the striations. In the next experiment a similar tube was taken, the water being replaced by bromine; this, too, showed the usual differences between the discharge at the two electrodes, and similar appearances were presented by a tube in which the electrodes were re-distilled arsenic and the gas arsenic vapour.

Another difficulty in the way of explaining the difference at the two electrodes by chemical action is that no difference seems to be made in the appearance when a strongly electronegative gas, such as chlorine, is substituted for a strongly electropositive one, such as hydrogen.

I next endeavoured to get rid of the electrodes altogether by trying to get a circular discharge in an exhausted re-entrant tube without any electrodes. For this purpose the primary was generally a piece of copper rod bent into a horse-shoe shape; the secondary circuit was an endless circular glass tube from which the air had been exhausted. A Leyden jar, charged by a Wimshurst machine, was discharged through the primary, and produced by induction an electromotive force round the exhausted tube. When the secondary was not shielded from the electrostatic induction of the primary, it was

filled with a uniform glow whenever the discharge passed through the primary circuit, but, when the electrostatic induction was shielded off by pieces of wet thin blotting paper connected to earth, no glow could be observed, though the wet blotting paper is not a sufficiently good conductor to shield off electromagnetic induction.

The maximum integral electromotive force round the secondary is shown to be VM/L , where V is the difference between the potentials of the coatings of the jar before discharge, L the coefficient of self-induction of the primary circuit, and M the coefficient of mutual induction between the circuits. Though in my experiments this was greater than the electromotive force requisite for a discharge through gas at the same density between terminals separated by the length of the tube, not the faintest glow could be detected. All my efforts to get a discharge through the secondary have so far been unsuccessful,* and I feel sure that the ease of getting a discharge without electrodes, say by the motion of the upper regions of the earth's atmosphere across the lines of magnetic force, has been much over-estimated. Until, however, we have got a discharge without electrodes through nothing but the gas itself, we are unable to say whether the passage of the discharge from the positive to the negative electrode which occurs in gases is a consequence of having matter in two states in the path of the discharge, or whether it is an example of a more general law, that, whenever tubes of electrostatic induction shorten in a conducting circuit, they do so in the direction of the electric displacement.

In conclusion, I have much pleasure in thanking Mr. Bartlett and Mr. Everett for the assistance they have given me in the course of this investigation.

II. "Note on the Present State of the Theory of Thin Elastic Shells." By A. E. H. LOVE, M.A., St. John's College, Cambridge. Communicated by LORD RAYLEIGH, Sec. R.S. Received January 3, 1891.

In a paper read before the Royal Society in February, 1888, and published in 'Phil. Trans.,' A, of that year, I advanced a theory of the mode of deformation that takes place when a thin shell is vibrating. The theory was founded on the form of the potential energy function, obtained by a method adapted from that of Kirchhoff for plates. It appears that, in case there are no surface-stresses on the faces of the shell, this function consists of two terms, of which one contains a certain function W_2 and the thickness $2h$ as factors, and

* Since this paper was sent in to the Royal Society, I have succeeded in getting a discharge without electrodes through a tube about 45 cm. in circumference. The discharge did not exhibit any signs of stratification.—Jan. 23, 1891.

the other contains a function W_1 and h^3 as factors. The term W_2 depends entirely on quantities σ_1 , σ_2 , ϖ , expressing the extension of the middle surface, while the form given for W_1 contained only quantities expressing the changes of curvature. Some previous theories proceeded as if W_1 alone occurred, and, in fact, this was the case with a paper by Lord Rayleigh in 'Proceedings of the London Mathematical Society,' vol. 13, 1882, on the "Infinitesimal Bending of Surfaces of Revolution." In the latter paper, a theory of the vibrations of bells was founded on an assumed type, viz., it was assumed that the middle surface remains unstretched. In my paper it was shown that this solution of Lord Rayleigh's fails to satisfy the boundary conditions which hold at the free edges of the bell, and further that it is, in general, impossible to satisfy these conditions, except by taking account of the extension. I, therefore, proposed to substitute for the theory of Lord Rayleigh one in which extension of the middle surface of the bell is recognised as taking place, and I did not see how to avoid the conclusion that the term W_1 must be rejected, and the term W_2 retained, for the purpose of forming the differential equations and boundary conditions that govern the motion, in other words, that the extension practically determines everything—the mode of vibration and the pitch.

Since that paper was written the subject has been investigated by Lord Rayleigh, Mr. Basset, and Professor Lamb, and the results of their work make it necessary to abandon the theory proposed. I had overlooked a circumstance which shews that my theory of extensional vibrations is incapable of giving the gravest modes of vibration of which the shell is capable, viz., the period given by Lord Rayleigh's solution, founded on the assumed type, is, in the limiting case of vanishing thickness, infinitely long in comparison with the gravest extensional period. Now it is a general dynamical theorem that the tone obtained by assuming the type cannot be graver than the gravest tone natural to the system, and it follows that the mode of deformation corresponding to the gravest tone is not included among the extensional modes. This was pointed out by Lord Rayleigh in a paper read before the Society in December, 1888, and published in the 'Proceedings.' It had still to be shown, however, that vibrations mainly dependent on the bending could take place, and the boundary conditions be satisfied. Although this has not yet been done in any particular case, the suggestion thrown out by Mr. Basset* and Professor Lamb,† probably contains the solution of the

* "On the Extension and Flexure of Cylindrical and Spherical Thin Elastic Shells," 'Phil. Trans.,' A, 1890.

† "On the Deformation of an Elastic Shell," 'London Math. Soc. Proc.,' vol. 21, 1890.

difficulty. Each of these writers has shown that, in particular statical problems relating to cylinders, the quantities expressing the extension can be very small everywhere except in the neighbourhood of an edge, and there they may increase with such rapidity as to secure the satisfaction of the boundary conditions, the total potential energy due to extension, which varies as the surface integral of hW_2 over the middle surface, being, nevertheless, negligible in comparison with that due to bending, which varies as the surface integral of h^3W_1 . Mr. Basset and Professor Lamb both suggest that this may be the solution of the difficulty in the case of vibrations also, and their results point to a method of approximation which might be applied to the general case, and such that it could be verified by mathematical analysis that Lord Rayleigh's solution, founded on an assumed type, is actually a very close approximation to the state of things in any part of a vibrating bell not very close to a free edge.

It may be as well to point out what parts of the theory put forward in my paper specially require revision. (1.) On p. 500 the alteration suggested in Kirchhoff's theory is erroneous; the quantities u' , v' , w' are functions of α , β , and their differential coefficients must be introduced as by Kirchhoff, and afterwards neglected; this correction makes no difference to any of the results. (2.) On p. 503, Art. 4, the "products" neglected are such as occur in the equations when account is taken of the fact that the axes of reference are really not in fixed directions. If they had been retained, the part of the potential energy which is multiplied by h^3 would have contained terms depending on the extension as well as terms depending on the bending. Mr. Basset has obtained, by a different method, the form of this function for cylindrical and spherical shells, with these terms expressed. It follows that the form given for the potential energy in equation (12), p. 505, is only correct in case either (a) the shell is unextended, when its second line vanishes, or (b) the extension is the important thing, when its first line may be neglected; but it would most probably be sufficiently exact for the application of a method of approximation. (3.) The first paragraph of Art. 13, p. 521, is wrong, and so are all other paragraphs to the same effect; viz., it is incorrect to conclude that, because σ_1 , σ_2 , π do not everywhere vanish, therefore W_1h^3 is infinitely small in comparison with W_2h . It appears, on the contrary, that the values of σ_1 , σ_2 , π can be very small indeed everywhere except close to the edges, in such a way that the integral of W_2h , taken over the middle surface, is very small in comparison with that of W_1h^3 .

The remainder of the paper must be understood as giving a theory of the extensional vibrations of the shell. Such vibrations undoubtedly can exist, but they would be difficult to excite, and the theory of them has no application to vibrating bells under ordinary conditions.

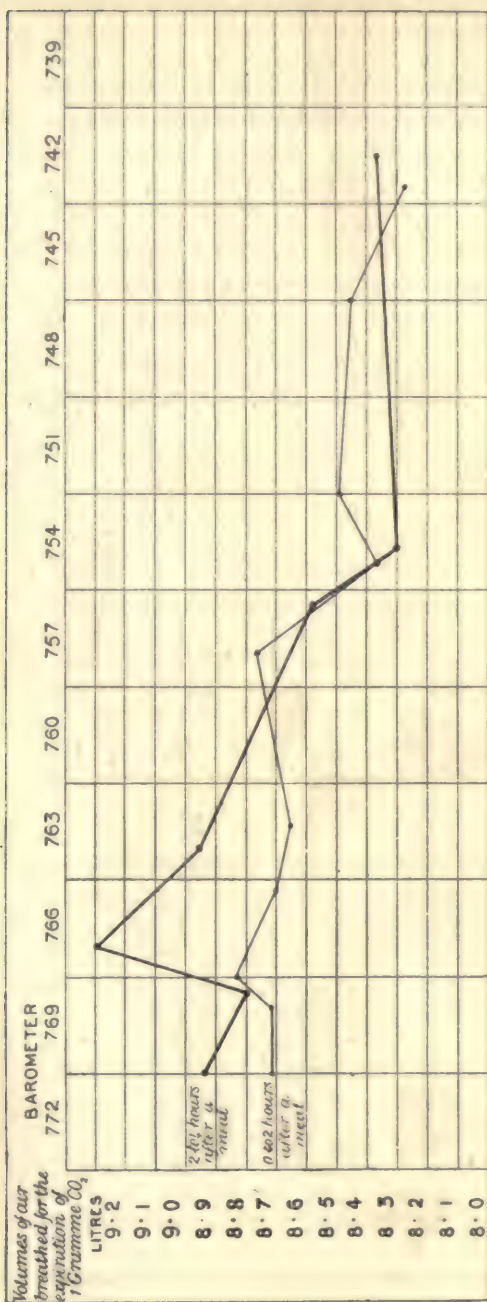
III. "On the Chemical Phenomena of Human Respiration while Air is being re-breathed in a closed Vessel." By WILLIAM MARCET, M.D., F.R.S. Received January 3, 1891.

In June, 1889, I had the honour of communicating a paper to the Royal Society, which appeared subsequently in the 'Philosophical Transactions' for 1890.* In this paper it was shown that the volumes of air breathed to form in the body and expire a given weight of carbonic acid exhibited a distinct tendency to fall with a local subsidence of atmospheric pressure, and *vice versâ*. Since then an additional series of experiments, to which my present assistant, Mr. E. Russell, kindly submitted, confirmed this result. Fifteen experiments were made from 0 to 2 hours after a meal, and fifteen also from 2 to 4 hours after a meal. The results are disposed as follows, in the form of a chart (see next page), in which the curves for the volumes of air breathed to expire 1 gram CO_2 are seen to fall most distinctly from nearly 767 mm. pressure to 742 mm.

The object of the present investigation was to ascertain the effects produced on the chemical phenomena of respiration by re-breathing a given volume of air for a given time, and I gladly acknowledge the valuable aid of my assistant, Mr. Edward Russell, F.C.S., in the course of this inquiry. It was obvious that I could not risk the health of those who, together with myself, submitted to experiment; hence the necessity of limiting the duration of the time for re-breathing air, and I selected for this purpose a period of five minutes. A certain volume of air to be re-breathed was settled upon from the beginning, and it was decided to take 35 litres, measured under atmospheric pressure with every care. This air was held in a bell-jar of a capacity of 40 litres, and supplied with a scale, a thermometer, and an oil-gauge; it was maintained in suspension by a counter-poise, while immersed in a trough full of salt water. The bell-jar was, moreover, possessed of a regulating apparatus, keeping it in perfect equilibrium in every position, as it rose or fell in the tank.

Four persons submitted to these experiments. I head the list, with six complete experiments. Next, my assistant, Mr. Russell, had ten complete experiments made upon himself; a former assistant, Mr. Hoskins, F.C.S., in accordance with my request, kindly submitted to eleven experiments, and, finally, W. Alderwood, my laboratory attendant, who has been in my service for seven years, and is well qualified for this kind of work, had ten experiments made upon him.

* 'Phil. Trans.,' B, 1890, "A Chemical Inquiry into the Phenomena of Human Respiration."



The results from all these experiments will be found disposed in the form of tables (pp. 113—116).

I shall first give a short account of the method adopted in the present inquiry, then describe the experiments, and finally state the results with which they have been attended. Two bell-jars were made use of.

The air, in every one of the experiments quoted in this paper, was inspired through the nose and expired through the mouth, a mode of breathing easy to acquire, and soon becoming perfectly natural; the person under experiment assumed the recumbent position in a deck-chair, with the feet resting on a stool.

It was necessary to begin by determining the volumes of air and weights of carbonic acid expired normally, or in ordinary breathing, with the object of using these figures as standards for comparison. I need not say that every precaution was taken to obtain correct data on ordinary breathing. Next, the other bell-jar was supplied with atmospheric air to be re-breathed. A correction might have been introduced for the CO_2 naturally present, but from its small proportion this correction was thought unnecessary. On no occasion was the laboratory used for the evolution of acids or alkalis, and its ventilation was kept up by one or two open windows.

After re-breathing 35 litres of air during five minutes, the person under experiment was placed in communication with the other bell-jar, in such a manner that no air whatever was lost, or, in other words, while fresh air was *inspired*, the *expiration* following immediately the last expiration of re-breathed air was collected in the other bell-jar, now emptied of the expired air it formerly contained. While this bell-jar was being filled, the re-breathed air, from the other bell-jar (after its volume had been read, and temperature taken), was driven into an india-rubber bag faced with oil-skin, to prevent any loss of any CO_2 by diffusion. This bag had been kept flattened down between boards weighted with a piece of iron weighing 20 lbs., a precaution taken to empty perfectly the bag before it was used for storing the re-breathed air. The bell-jar, having thus discharged its contents, was ready to be used afresh.

The person expired from 34 to 38 litres of air immediately after the re-breathing stage of the experiment, and he was now placed in communication with the empty bell-jar; there was no loss of expired air through this passage from one bell-jar to another, and while fresh air was inhaled, the air expired was entirely collected, to the extent of from 34 to 38 litres, for subsequent analysis. A chronograph showed, to a second, the time required in the various stages of the experiment.

Thus the whole history of the effects of re-breathing air was obtained, being divided into four stages:—1st, natural respiration; 2ndly, air

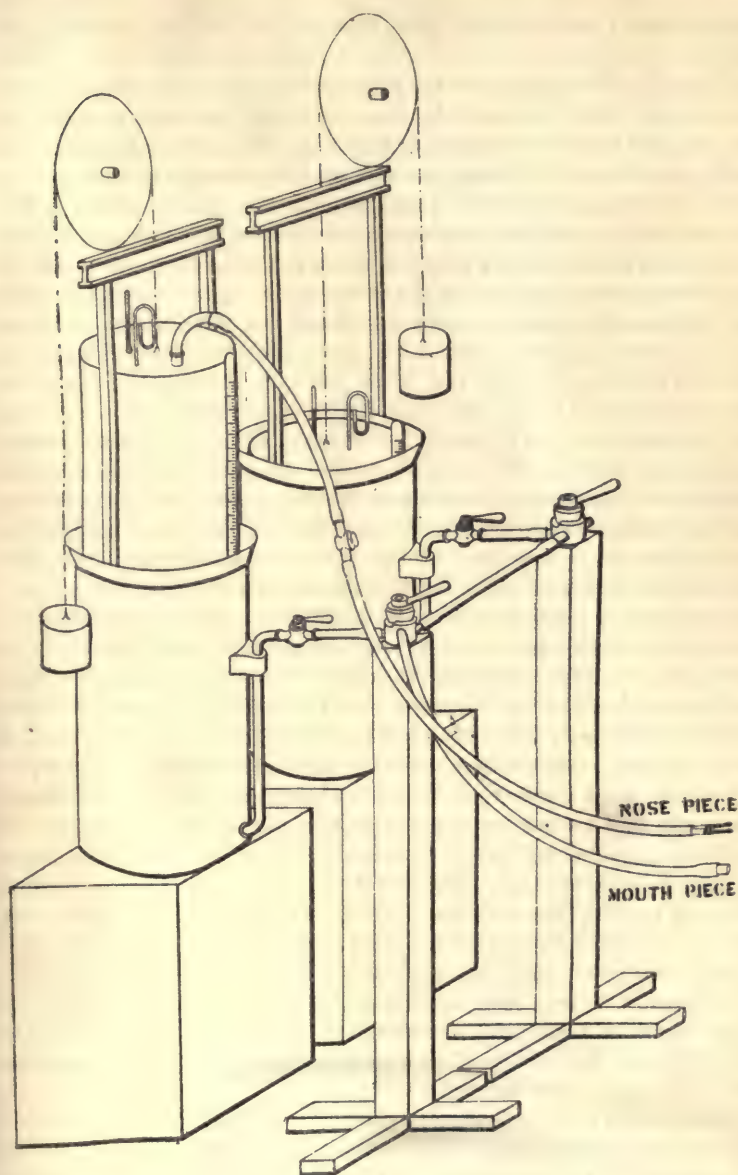
re-breathed; 3rdly, air expired immediately after re-breathing and with the inspiration of fresh air; 4thly, air breathed while no longer under the direct influence of re-breathing.

The above is a general sketch of the investigation. I must now beg leave to go into the details of the work. The diagram on the next page illustrates the disposition of the instrument.

The person under experiment, in the recumbent position in a deck-chair, held in his right hand an india-rubber tube connected with the bell-jar through a double-way cock, as I have explained in my last paper to the Royal Society. The cock was turned in such a position that the air inspired through the nose was expired into the open air, a little flag showing the movement of the expired air through the tube. The experiment begins with the operator expiring through this tube into the external air. When respiration has become perfectly quiet and regular, the double-way cock is turned during an inspiration, and the air of the next expiration is collected in the bell-jar, which begins its ascending course. At the same time the hands of a chronograph are set in motion. After about 36 litres of air have been expired, the tube leading into the bell-jar is closed at the end of an expiration, and the chronograph is stopped.

Some trouble was experienced in obtaining similar volumes of air expired in a given time, say, about every seven minutes. I have come to the conclusion that most people do not breathe, even when perfectly still, exactly the same volume of air in a given time, and after an experience of many years, it was found that the best plan was to repeat the breathing two or three times, or more, in succession, and to take, according to circumstances, either the mean of the different experiments, or the result of the last made. Any experiment differing widely from the others was rejected. The air collected finally was read off on the scale attached to the bell-jar, its temperature was taken, the barometer read, and the air was analysed for the determination of its carbonic acid, by the same method as that which has been described in the paper in the 'Philosophical Transactions' already referred to. The air left in the bell-jar was then driven out, and the jar made ready for further use.

The next part of the work is the re-breathing. Perhaps half an hour has elapsed since air was first collected for the determination of CO_2 in normal breathing; during that time the person under experiment has remained perfectly still in the deck-chair. The bell-jar, which has not yet been used, is now thoroughly rinsed with common air and filled with air to the extent of 35 litres. It carries an india-rubber tube, connected with the dome of the jar and supplied at the end with a fork-shaped nose-piece (see diagram). This nose-piece has been ascertained to fit the nostrils of the operator, air-tight; a second india-rubber tube, with a mouth-piece at one end, is connected



at the other with a U-shaped pipe, opening under the bell-jar in the ordinary way. The bell-jar holding 35 litres of air is perfectly counterpoised, so that the operator moves it up and down unconsciously in the act of respiration, while the oil-gauge on the bell-jar registers

barely from 1 to 2 mm. of difference of pressure, which is inappreciable.

Placing the nose-piece in his nostrils, the operator breathes through his mouth for a few seconds, then he takes the mouth-piece in his mouth, and inhales the air of the bell-jar through the nose-piece, the bell-jar falling; at that very instant the chronograph is started. The next expiration is from the mouth through the U-tube into the bell-jar, and so on, the air re-breathed circulating through the bell-jar. After five minutes have elapsed, every attention is given to stop the inspiratory tube and arrest the chronograph at the very end of an expiration, while another assistant opens the double-way cock, connected through tubing with the operator, and disposed so as to lead the air now expired into the other bell-jar; the operator drops the nose-piece and takes an inspiration of fresh air, through the nose, then he expires out of the mouth into the empty bell-jar. He was, perhaps, beginning to feel a little uncomfortable; sometimes a slight sensation of want of air was experienced, but not always, and one of my subjects hardly noticed any effect. I think I was affected most of the four who submitted to experiment, although it repeatedly happened that I felt no discomfort of any kind, beyond perhaps a slight want of air.

Fresh air is inhaled with an undoubted sensation of comfort, and the volume of this air is in marked excess of the volume inhaled in ordinary breathing. During the first two or three minutes, large volumes of fresh air are inspired, then the breathing quickly subsides, and before 36 or 37 litres have been expired it has apparently resumed its usual rate, with the disappearance of all feeling of discomfort.

I have stated above that the air re-breathed had been transferred from the bell-jar into an india-rubber bag, allowing the bell-jar to be utilised for collecting the air expired in the last stage of the experiment. The india-rubber tube and double-way cock were so arranged that by turning the cock the operator was placed in connexion with the empty bell-jar, and during an inspiration of fresh air the cock was turned, when the expired air was directed into that bell-jar.

The rate of breathing had now become all but natural, or the same as at the beginning of the experiment, giving indications that the effects of re-breathing had apparently passed away; this question was to be settled by the analyses.

There were consequently four different samples of expired air to be submitted to analysis for the determination of the carbonic acid they contained. The first sample was from air expired normally, the second from air re-breathed, the third from air expired immediately after re-breathing, the fourth from air expired after apparent recovery from the effects of breathing impure air.

By the time breathing in the closed vessel had commenced, the air expired normally had already been shaken with barium hydrate;

samples from the bag and other two bell-jars were treated in the same way. The next day the barium carbonate had subsided, and the clear fluid was titrated according to Pettenkofer's method. The reductions to dryness, to 0° and 760 mm., were speedily made with the help of the table I have given in the paper previously referred to.

Let us now follow the changes occasioned by the re-breathing of 35 litres of air for a period of 5 minutes; there were a few additional seconds included, as the re-breathing had to be stopped at the end of an expiration, which of course might not exactly correspond with a lapse of five minutes. The following are the mean percentages of CO₂ contained in the bell-jar after its air had been re-breathed:—

Myself.....	after 5 m.	2 sec.	3·42	per cent CO ₂ .
Mr. Russell.....	„	5 m. 4 sec.	3·87	„ „
Mr. Hoskins	„	5 m. 10 sec.	3·44	„ „
W. Alderwood	„	5 m. 5 sec.	3·29	„ „

consequently in every case the air was becoming considerably vitiated; yet it was only in the last minute that an unpleasant sensation, if any, was felt.

If we compare the amount of carbonic acid expired by re-breathing 35 litres of air for five minutes with the amount of carbonic acid which would have been expired in the same time in ordinary breathing, we find invariably less CO₂ in the re-breathed air than in normal respiration; this is shown clearly in the following table, in which the CO₂ expired in ordinary breathing has been calculated for the time taken in the re-breathing stage of the experiment.

	Time.	CO ₂ in re-breathed air.	CO ₂ expired normally in same time.	Relations of CO ₂ expired in re-breathed air to CO ₂ expired in natural breathing.
Myself	5 m. 2 sec.	2·135	2·224	1 to 1·041
Mr. Russell	5 m. 4 sec.	2·418	2·797	1 to 1·157
Mr. Hoskins.....	5 m. 10 sec.	2·151	2·428	1 to 1·221
W. Alderwood....	5 m. 5 sec.	2·066	2·221	1 to 1·075
Mean	5 m. 5 secs.	2·192	2·417	1 to 1·123

It follows that there is always less carbonic acid expired in a given time when air is re-breathed than in ordinary breathing. In the present experiments the mean proportions varied for four different persons between 1 to 1·041 and 1 to 1·221; or, in other words, a mean of 9·3 per cent. carbonic acid which would have been expired in a certain time in ordinary breathing is found to have disappeared in 35 litres of air re-breathed during the same time.

[This amounts to 225 c.c. CO_2 , which have been retained in the blood; but it occurs to me that less oxygen may possibly be consumed from re-breathed air than from fresh air, although in my experiments re-breathing is hardly carried far enough to admit of such a contingency.—*Jan. 22.*]

We find, by a consideration of the next table, that the reduced elimination of carbonic acid in re-breathed air is regulated in a marked degree by the weights of CO_2 expired in ordinary breathing.

	CO_2 produced by re-breathing.	CO_2 expired in ordinary breathing in the same time.
W. Alderwood.....	2·066 grams	2·221 grams
Myself.....	2·135 "	2·224 "
Mr. Hoskins.....	2·151 "	2·428 "
Mr. Russell.....	2·418 "	2·797 "

Thus it is seen that the CO_2 in re-breathed air and in ordinary breathing increase together from the lowest to the highest figures. This might have been expected, as the whole experiment must be controlled more or less by the phenomena of ordinary breathing for each of the persons under experiment.

There is another point of interest to be noticed with reference to the re-breathed air in the present experiment—the volume of this air, which originally was 35 litres, is no longer 35 litres at the conclusion of the experiment, but has undergone a slight reduction. The enquiry into this portion of the subject was not found so simple as it appeared to be at first, and, as the work progressed, precautions against errors had to be taken which had not been apparent until a late period of the investigation. I, therefore, prefer to leave this part of the subject for future consideration.

It has been stated that after re-breathing for five minutes the air of the bell-jar, and then admitting fresh air into the lungs, an increased volume of air was inhaled attended with the expiration of a greater amount of carbonic acid than in ordinary breathing. This will be seen in the following table, showing, for the same lapse of time, the mean results obtained on four different persons for ordinary respiration and while inhaling fresh air, immediately after the re-breathing stage of the experiment.

A consideration of these tables shows, with reference to the CO_2 expired, that there was invariably an excess, after re-breathing air for five minutes, over the weight expired in the same time in normal respiration; the mean relation being 1 to 1·237. In the case of

CO₂ before re-breathing calculated on time after re-breathing.

	Time.	Before re-breathing. CO ₂ , grams.	After re-breathing. CO ₂ , grams.	Relation.
Self	5 m. 40 sec.	2·474	3·134	1 to 1·267
Mr. Russell	4 m. 41 sec.	2·507	3·224	1 to 1·294
Mr. Hoskins.....	5 m. 26 sec.	2·802	3·614	1 to 1·290
W. Alderwood....	6 m. 57 sec.	3·018	3·311	1 to 1·097
Mean	5 m. 41 sec.	2·700	3·326	1 to 1·237

Litres of Air expired before re-breathing calculated on time after re-breathing.

Before re-breathing.	After re-breathing.	Relation.
26·56	33·93	1 to 1·278
24·96	35·70	1 to 1·430
26·24	35·27	1 to 1·344
29·17	34·34	1 to 1·177
26·73	34·81	1 to 1·307

W. Alderwood, who was the least affected of the four persons under experiment, the excess of CO₂ after re-breathing, amounting to 1 to 1·097, is the smallest. A similar remark applies to the volumes of air expired; they are invariably increased after re-breathing, or while the person under experiment is still under the influence of the want of air; the mean relation is 1 to 1·307; again, in the case of W. Alderwood the increase is the smallest, the proportion amounting to 1 to 1·177.

The excess of CO₂ and of air expired when fresh air is breathed immediately after the re-breathing stage of the experiment must be due in a great measure to the increased amount of carbonic acid retained in the blood, together with an instinctive desire of taking into the lungs increased volumes of air, in order to rid the blood of the carbonic acid it has retained.

We now have to deal with the air expired finally or in the bell-jar filled at the termination of the experiment. The mean volumes of air and weights of CO₂ expired per minute will be seen to approximate to the corresponding volumes and weights expired in ordinary breathing to such an extent that respiration may be considered as having returned to the normal condition.

Table showing the Volumes of Air and Weights of CO₂ expired in the final stage of the experiment compared with the corresponding volumes and weights expired normally.

	Vol. air expired per minute unreduced.		Weight CO ₂ expired per minute.		Difference.	
	Normal.	Last stage of experiment.	Normal.	Last stage of experiment.	Vols. air.	Weights.
Self	4·687	4·935	0·442	0·457	+0·248	+0·015
Mr. Russell ...	5·195	5·546	0·552	0·550	+0·351	—0·002
Mr. Hoskins ..	4·954	4·986	0·470	0·454	+0·032	—0·016
W. Alderwood.	4·197	4·276	0·437	0·422	+0·079	—0·015
Means	4·758	4·936	0·475	0·471	+0·178 = 3·6 per cent. increase vol. air.	—0·004

This table shows unmistakably that the respiration had again become normal before or by the end of the last stage of the experiment; the CO₂ is all but exactly the same, while there is a very slight increase by 3·6 per cent. in the volume of air expired, indicating that there was perhaps an instinctive tendency to continue breathing a volume of air slightly larger than usual, although the CO₂ expired was the same as in normal respiration.

The following are the results obtained from the present inquiry:—

1. On re-breathing air in a closed vessel less carbonic acid is expired within a given time than in ordinary breathing.

2. Those persons who emit most CO₂ in re-breathed air are those who expire most air and CO₂ in the same time in ordinary breathing, and *vice versa*.

3. On re-breathing 35 litres of air in a closed vessel for a period of five minutes, the volume of this air undergoes a slight reduction.

4. When fresh air is taken into the lungs immediately after re-breathing air in a closed vessel, the volumes of air breathed and weights of CO₂ expired are greater than in ordinary breathing.

5. The effects produced on the chemical phenomena of respiration by re-breathing 35 litres of air in a closed vessel for a period of five minutes have passed away in less than six minutes after the breathing of fresh air has been resumed.

It may be added that the number of experiments is insufficient to admit of any inquiry into the influence of barometric pressure on respiration.

The tables showing the general results of the experiments are as follows:—

Dr. Marcet under experiment.

No. of exp.	Lab. temp.	Bar. mm.	Normal respiration.			Air re-breathed.		Respiration immediately after re-breathing air.			Respiration at end of experiment.		
			CO ₂ expired in ordinary breathing per minute.	Per cent. vol. CO ₂ .	Vol. in litres of air expired for 1 gram CO ₂ .	Time of re-breathing.	Per cent. vol. CO ₂ in re-breathed air.	CO ₂ expired immediately after breathing, per minute.	Per cent. vol. CO ₂ .	Vol. in litres of air expired immediately after re-breathing for 1 gram CO ₂ .	Final CO ₂ expired per minute.	Per cent. vol. CO ₂ .	Vol. in litres of air expired for 1 gram CO ₂ .
1	67°·2	767·0	gram. 0·437	5·09	9·96	min. sec. 5 2	3·41	gram. 0·492	5·21	9·73	gram. 0·448	5·09	9·96
2	67°·5	745·5	0·456	5·49	9·24	4 56	3·13	0·514	5·55	9·15	0·450	5·60	9·06
3	58·7	744·0	0·445	5·09	9·97	5 0	3·31	0·510	5·22	9·72	0·453	4·92	10·31
4	63·8	758·8	0·429	5·18	9·80	5 6	3·52	0·528	5·25	9·66	0·479	5·22	9·71
5	67°·2	759·5	0·423	5·03	10·08	5 3	3·81	0·579	5·21	9·73	0·482	5·25	9·66
6	67°·6	758·6	0·463	5·11	9·92	5 3	3·36	0·488	5·01	10·12	0·428	4·76	10·66
Means	65·3	755·5	0·442	5·16	9·83	5 2	3·42	0·518	5·24	9·68	0·457	5·14	9·89

Volumes reduced to 0° and 760 mm.

Mr. Russell under experiment.

No. of expt.	Labr. temp.	Bar.	Normal respiration.			Air re-breathed.		Respiration immediately after re-breathing air.			Respiration at end of experiment.		
			CO ₂ expired in ordinary breathing per minute.	Per cent. vol. CO ₂ .	Vol. in litres of air expired for 1 gram CO ₂ .	Time of re-breathing.	Per cent. vol. CO ₂ in re-breathed air.	CO ₂ expired immediately after re-breathing, per minute.	Per cent. vol. CO ₂ .	Vol. in litres of air expired immediately after re-breathing, for 1 gram CO ₂ .	Final CO ₂ expired per minute.	Per cent. vol. CO ₂ .	Vol. in litres of air expired for 1 gram CO ₂ .
1	68.0	755.1	gram. 0.508	6.07	8.34	min. sec. 5 5	gram. 3.46	gram. 0.632	5.81	8.72	gram. 0.487	5.58	9.08
2	54.0	750.8	0.553	6.15	8.25	5 0	3.88	0.796	5.61	9.04	0.586	5.48	9.27
3	57.5	763.0	0.523	5.85	8.66	5 10	3.71	0.861	7.03	7.21	0.572	5.37	9.44
4	60.0	756.7	0.555	5.80	8.75	5 2	3.85	0.810	5.69	8.92	0.558	5.54	9.16
5	61.7	751.5	0.513	5.90	8.60	5 7	3.68	0.718	5.34	9.51	0.454	5.35	9.47
6	59.6	750.2	0.571	5.56	9.12	5 4	4.00	0.780	5.13	9.88	0.491	4.93	10.29
7	61.0	745.7	0.549	5.88	8.63	5 2	4.16	0.766	5.55	9.14	0.543	5.50	9.22
8	63.5	748.7	0.567	5.85	8.67	5 5	4.16	0.841	5.58	9.09	0.538	5.34	9.50
9	67.5	758.2	0.590	5.86	8.66	5 3	3.84	0.782	5.46	9.29	0.584	5.33	9.51
10	68.7	755.3	0.592	5.63	9.01	5 3	3.97	0.773	5.36	9.46	0.615	5.38	9.43
Means	62.1	753.5	0.552	5.85	8.67	5 4	3.87	0.776	5.66	9.03	0.550	5.38	9.44

Volumes reduced to 0° and 760 mm.

Mr. Hoskins under experiment.

No. of exp.	Lab. temp.	Bar.	Normal respiration.			Air re-breathed.		Respiration immediately after re-breathing air.			Respiration at end of experiment.		
			CO ₂ expired in ordinary breathing per minute.	Per cent. vol. CO ₂ .	Vol. in litres of air expired for 1 gram CO ₂ .	Time of re-breathing.	Per cent. vol. CO ₂ in re-breathed air.	CO ₂ expired immediately after re-breathing per minute.	Per cent. vol. CO ₂ .	Vol. in litres of air expired after re-breathing for 1 gram CO ₂ .	Final CO ₂ expired per minute.	Per cent. vol. CO ₂ .	Vol. in litres of air expired for 1 gram CO ₂ .
1	68.2	757.0	gram. 0.464	5.08	10.00	min. sec. 5 7	gram. 3.55	gram. 0.548	5.00	10.15	gram. 0.425	5.01	10.12
2	69.5	760.1	0.454	5.03	10.08	5 0	3.68	—	—	—	0.414	5.53	9.18
3	68.4	766.5	0.458	5.14	9.88	5 3	2.94	0.547	4.98	10.19	0.436	4.96	10.24
4	57.8	767.1	0.490	5.54	9.15	5 5	3.37	0.608	5.39	9.42	0.424	5.28	9.61
5	56.0	762.2	0.472	5.57	9.12	5 5	3.46	0.657	5.25	9.58	0.460	5.37	9.46
6	55.5	750.2	0.501	5.33	9.53	5 0	3.46	0.565	5.00	10.15	0.418	4.84	10.48
7	54.2	753.5	0.426	4.47	11.34	5 0	3.10	0.582	4.42	11.47	0.408	4.34	11.70
8	57.0	745.7	0.439	4.65	10.91	6 3	3.86	0.669	4.74	10.70	0.546	4.49	11.29
9	59.0	743.4	0.488	5.47	9.28	5 3	3.43	0.713	4.95	10.25	—	—	—
10	59.0	743.7	0.505	4.99	10.17	5 10	3.41	0.593	4.98	10.18	0.545	4.87	10.40
11	56.5	751.0	0.476	5.19	9.76	5 10	3.55	0.646	4.69	10.81	0.462	4.64	10.88
Means	60.3	754.6	0.470	5.01	9.93	5 10	3.44	0.613	4.94	10.29	0.454	4.93	10.34

Volumes reduced to 0° and 760 mm.

William Alderwood under experiment.

No. of exp ^t .	Lab ^y . temp.	Bar.	Normal respiration.			Air re-breathed.		Respiration immediately after re-breathing air.			Respiration at end of experiment.		
			CO ₂ expired in ordinary breathing per minute.	Per cent. vol. CO ₂ .	Vol. in litres of air expired for 1 gram CO ₂ .	Time of re-breathing.	Per cent. vol. CO ₂ in re-breathed air.	CO ₂ expired immediately after re-breathing per minute.	Per cent. vol. CO ₂ .	Vol. in litres of air expired immediately after re-breathing for 1 gram CO ₂ .	Final CO ₂ expired per minute.	Per cent. vol. CO ₂ .	Vol. in litres of air expired for 1 gram CO ₂ .
1	65.2	758.5	gram. 0.477	5.90	8.60	min. sec. 5 0	gram. 3.40	gram. 0.476	5.68	8.93	gram. 0.474	5.61	9.04
2	64.5	746.0	0.409	5.65	8.98	5 7	3.44	0.502	4.88	10.40	0.410	5.06	10.02
3	53.7	755.8	0.494	5.79	8.76	5 0	3.41	0.560	5.16	9.84	0.478	5.17	9.82
4	60.0	756.0	0.453	5.93	8.55	5 5	3.25	0.507	5.27	9.62	0.449	5.51	9.20
5	61.0	760.0	0.505	5.65	8.98	5 3	3.41	0.573	5.16	9.84	0.435	5.64	9.00
6	59.5	750.2	0.431	5.67	8.95	5 10	3.41	0.445	5.39	9.42	0.418	5.38	9.42
7	59.5	758.0	0.376	5.59	9.08	5 5	2.96	0.420	5.12	9.92	0.351	5.17	9.82
8	62.0	759.2	0.367	5.42	9.37	5 7	3.16	0.376	5.19	9.77	0.359	5.61	9.04
9	64.0	757.9	0.430	5.55	9.14	5 6	3.26	0.453	5.08	9.98	0.457	5.28	9.60
10	65.0	756.0	0.425	5.64	8.99	5 5	3.20	0.444	5.36	9.46	0.385	5.27	9.62
Means	61.4	755.8	0.437	5.68	8.94	5 5	3.29	0.476	5.23	9.72	0.422	5.37	9.46

Volumes reduced to 0° and 760 mm.

The foregoing tables suggest the following remark :—" The volumes of air expired for 1 gram CO_2 immediately after re-breathing air vary but slightly from the corresponding volumes of air emitted in ordinary breathing ; in every case except one, the volumes of air are a little higher immediately after re-breathing."

Presents, January 15, 1891.

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M. E. Gray. Mr. W. T. Thiselton Dyer, F.R.S.

January 22, 1891.

THE ASTRONOMER ROYAL, V.P.R.S., in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

- I. "On the Unsymmetrical Distribution of Terrestrial Magnetism." By HENRY WILDE, F.R.S. Received November 20, 1890.

[Publication deferred.]

- II. "The Passive State of Iron and Steel. Part II." By THOS. ANDREWS, F.R.S.S.L. and E., M.Inst.C.E. Received October 24, 1890.

In Part I of this research ('Roy. Soc. Proc.,' vol. 48, p. 116), the author showed the influence of magnetisation on the passive state of iron and steel, and he has now the pleasure of communicating to the Royal Society the results of a further study of certain temperature and other conditions affecting the passivity of these metals in concentrated nitric acid. The experiments of Series III, in this paper, relate to the effect of temperature, and the observations of Series IV refer to the influence exerted by nitric acids, of varied concentration, on the passive condition of iron and steel.

SERIES III.

Effect of Temperature on the Passivity of Iron and Steel.

The bars selected for these observations were unmagnetised polished rods, which had been previously drawn cold through a wire-draw; a pair of bars of each metal were cut adjacently from one longer bar, and then placed securely in the wooden stand, W; each bar was $8\frac{1}{4}$ inches long, 0.261 diameter. The U-tube containing $1\frac{1}{4}$ fluid oz. of nitric acid, sp. gr. 1.42, was rigidly placed in an arrangement as shown on fig. 3. One limb, A, was surrounded by a tank containing water, the other limb, B, by a tank of the same capacity, containing powdered ice; the arrangement was such that the water-tank could be heated by a

Bunsen burner, and its temperature slowly raised, whilst the ice-tank was kept full of powdered ice. A non-conductor of wood was put between the ends of the two tanks so as to prevent the melting of the ice; the bottom or bent portion of the U-tube was also enclosed in a thick non-conductor of wood. A thermometer, T, was placed in the water-tank. The bars were in circuit with the galvanometer, and soon after immersing them in the nitric acid heat was applied to the water-tank, and the temperature of the nitric acid in that limb of the U-tube slowly raised to the temperatures required, whilst the acid in the other limb of the U-tube was meanwhile maintained at a temperature of 32° F.

The arrangement will be understood on reference to fig. 3, and the electro-chemical results obtained are graphically recorded on Diagram L.

FIG. 3.

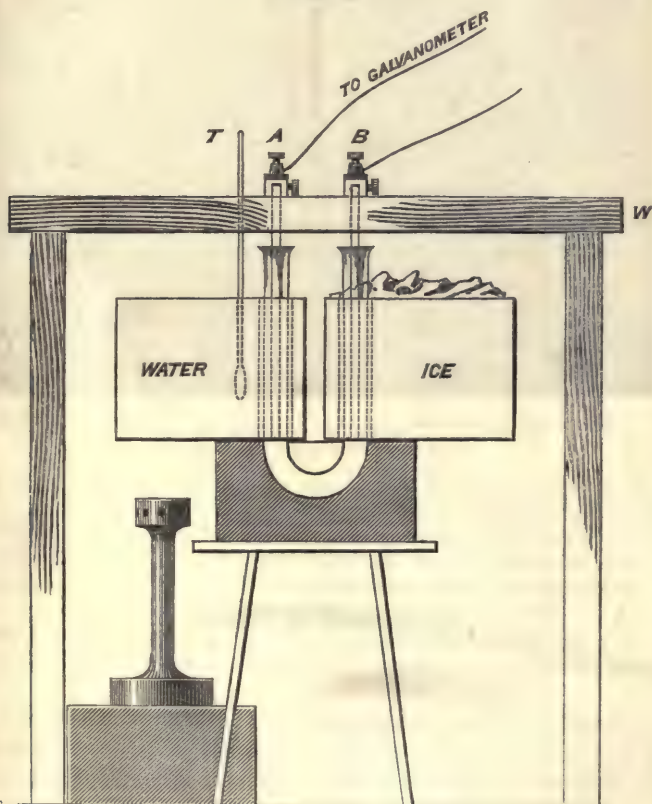
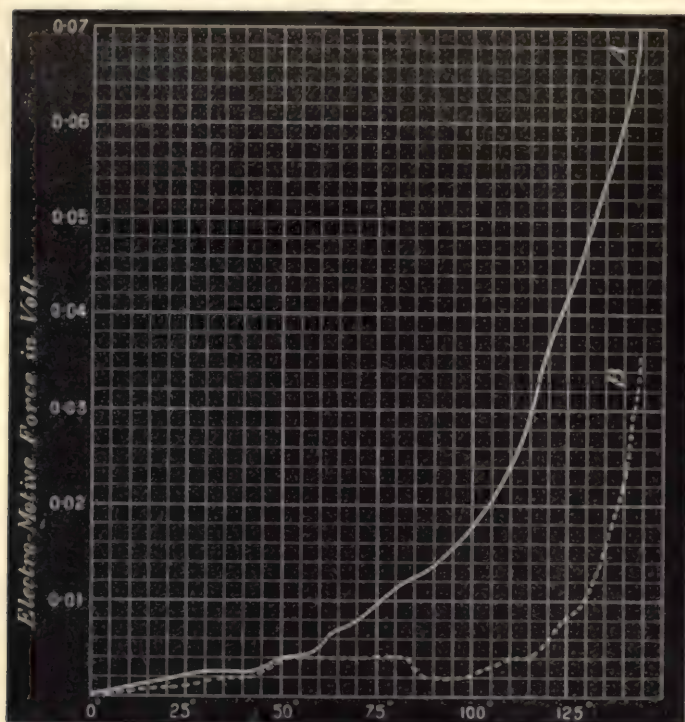


DIAGRAM I.

Current between two bright "passive" bars of the same composition, one in warm, the other in cold, nitric acid sp. gr. 1.42.

The electro-chemical position of the bar in the warm nitric acid was positive.



Difference of temperature between the nitric acid in Tubes A and B, see Fig. 3, in degrees Fahrenheit.

Curve A gives the E.M.F. between two wrought iron bars, and Curve B gives the E.M.F. between two cast steel bars under the conditions recorded.

The above experiments indicate that the wrought iron was less passive in the warm nitric acid than the soft cast steel; the average E.M.F. of 94 observations with wrought iron was 0.030 volt; whereas, in the case of the 94 observations on cast steel, the average E.M.F. was only 0.010 volt.

It will be seen from the above diagram that the behaviour of the steel, under the conditions stated, was more irregular than that of the wrought iron.

In the whole of the above series of experiments on Diagram I the nitric acid was raised to a temperature of 175° F.; the cold nitric acid in the limb of the U-tube A remained perfectly colourless, and the steel or iron therein absolutely passive; but the steel or iron in the warm nitric acid in tube A commenced to be gradually acted upon as the temperature increased, a pale yellow tint beginning to appear in

the solution in the tube A shortly after commencement. When the temperature of about 170° to 175° F. was reached a faint evolution of gas in the form of bubbles was manifest, adhering to the steel, in the warm tube only. No powerful solvent action or violent evolution of nitric oxide gas, however, occurred in any of these experiments even up to the temperature of 175° F., and these experiments were not continued beyond this temperature. These results show that iron or steel does not fully lose its passivity up to a temperature even of 175° F., though the passivity is shown to have been considerably modified by temperature only. The critical point of temperature of transition from the passive to the active state is therefore higher than 175° F., and is shown in the experiments of Part I, Series II, Table II, to have been about 195° F.

SERIES IV.

The Passivity of Iron and various Steels increases with the Concentration of the Nitric Acid.

Schönbein considered that, "by immersing an iron wire in nitric acid 1.50 sp. gr., it became likewise indifferent to the same acid of 1.35 sp. gr.," and to all outward appearance this is so.

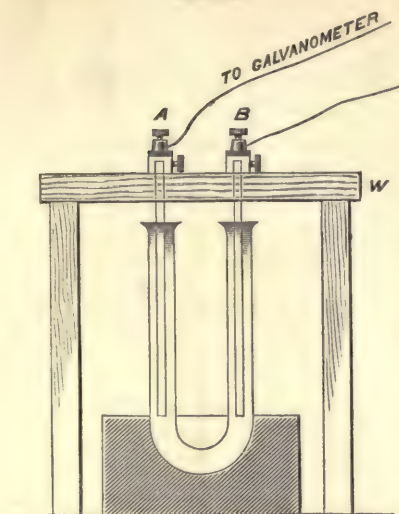
Scheurer-Kestner considered that the passivity of iron was not dependent on the greater or less degree of saturation of the acid. The author, however, ascertained by the delicate electro-chemical mode of experimentation employed, and hereafter referred to, that the passivity is materially influenced according to the concentration of the nitric acid.

The following experiments indicate that the property of passivity in iron is not absolutely fixed or static, but that its passivity is modified to a certain extent in relation to the strength of the nitric acid used. The general *modus operandi* was generally similar to that previously employed. Pairs of unmagnetised polished steel bars 6 inches long, and 0.310 inch diameter, each pair being of the same kind of steel, and cut adjacently from one longer bar, were placed as before in the wooden frame W, fig. 4, and then instantly and simultaneously immersed in nitric acids, of two different degrees of concentration, contained in the U-tube arrangement, one limb of the U-tube containing red fuming nitric acid of sp. gr. 1.50, the other containing nitric acid of sp. gr. 1.42, circuit being made through the galvanometer in the usual manner. The results, the average of repeated experiments in each case, are given on Table III, and show that the passivity of iron increases considerably with the strength of the nitric acid.

Table III.

Time from commencement of experiment.	Current between two bright "passive" wrought iron or various steel bars of the same composition, one in cold nitric acid sp. gr. 1.50, the other in cold nitric acid sp. gr. 1.42. The electro-chemical position of bar in weaker acid positive, except otherwise stated.				
	E.M.F. in volt.				
	Wrought iron.	Soft cast steel, combined carbon 0.57 per cent.	Hard cast steel, combined carbon 1.60 per cent.	Soft Bessemer steel, combined carbon 0.55 per cent.	Tungsten steel, combined carbon 1.75 per cent.
seconds.					
0	0.086	0.041	0.055	0.055	0.038
30	0.077	0.040	0.055	0.052	0.038
minutes.					
1	0.076	0.036	0.054	0.053	0.041
2	0.074	0.036	0.053	0.056	0.043
3	0.073	0.038	0.053	0.058	0.048
4	0.072	0.040	0.052	0.060	0.048
5	0.072	0.041	0.052	0.061	0.049
7½	0.071	0.041	0.050	0.067	0.050
10	0.069	0.041	0.049	0.071	0.050
15	0.066	0.040	0.048	0.074	0.050
20	0.064	0.037	0.046	0.077	0.049
25	0.062	0.035	0.043	0.074	0.049
30	0.060	0.034	0.042	0.072	0.048
35	0.059	0.033	0.040	0.071	0.048
40	0.058	0.031	0.038	0.071	0.047
45	0.056	0.030	0.038	0.070	0.047
50	0.055	0.029	0.036	0.068	0.046
55	0.054	0.029	0.036	0.067	0.046
hours.					
1	0.053	0.028	0.035	0.066	0.045
1½	0.051	0.025	0.034	0.061	0.044
2	0.049	0.022	0.033	0.058	0.043
2½	0.048	0.020	0.033	0.055	0.041
3	0.047	0.019	0.033	0.052	0.041
4	0.046	0.018	0.034	0.050	0.043
5	0.043	0.017	0.034	0.049	0.040
6	0.041	0.016	0.034	0.048	0.038
7	0.041	0.013	0.034	0.047	0.037
8	0.041	0.013	0.034	0.047	0.037
16	0.040	0.009	0.030	0.047	0.037
18	0.040	0.006	0.029	0.046	0.037
20	0.040	0.008	0.029	0.046	0.038
21	0.040		0.029	0.031	0.040
22	0.040		0.024		0.031
24	0.038		0.019		0.013
26	0.038		0.016		0.013
28	0.039		0.013		0.013
29	0.038		0.012		0.013
30	0.040		0.011		0.013
40	0.042		0.006		0.024
45					0.034

FIG. 4.



The steel rods selected for this set of experiments were of the kinds given on Table IV; they were drawn cold through a wortle, and were of the general physical properties and chemical composition given on Tables IV and V.

The reduction of E.M.F. towards the close was probably owing to partial diffusion between the two acids of different concentration.

The above results show that wrought iron was less passive in the weaker acid than most of the steels, the soft Bessemer steel being found similar in passivity to the wrought iron.

The average E.M.F. was as follows:—With wrought iron, 0·054 volt; soft cast steel, 0·028 volt; hard cast steel, 0·036 volt; soft Bessemer steel, 0·059 volt; tungsten steel, 0·039 volt.

Table IV.—Chemical Analysis of the Wrought Iron and Steel Bars used in the Experiments.

Description.	Combined carbon.	Silicon.	Sulphur.	Phosphorus.	Manganese.	Tungsten.	Iron (by difference).	Total.
	per cent.	per cent.	per cent.	per cent.	per cent.	per cent.	per cent.	per cent.
Wrought iron (Wortley best scrap)	trace	0·224	none	0·239	0·071	..	99·466	100·000
Soft cast steel	0·570	0·032	trace	0·066	0·147	..	99·185	100·000
Hard cast steel	1·600*	0·145	0·002	0·025	0·183	..	98·045	100·000
Soft Bessemer steel	0·550	none	0·032	0·175	0·216	..	99·027	100·000
Tungsten steel	1·750*	0·135	0·069	0·139	0·720	9·270	87·917	100·000

* By combustion.

The terms "soft" and "hard" relate only to difference of percentage of combined carbon, and not to their having undergone annealing or hardening processes.

Table V.—Physical Properties of the Wrought Iron and Steel Bars used in the Experiments.

Description.	Original.		Ultimate stress.		Fractured.		Stress per square inch of fractured area.	Extension in 10 inches.		Appearance of fracture.
	Size.	Area.	Total.	Per square inch of original area.	Size.	Area.		Inch.	Per cent.	
	inch.	sq. inch.	lbs.	tons.	inch.	sq. inch.	lbs.			per cent.
Wrought iron (Wortley best scrap)	0·296	0·0688	6,028	87,618 = 39·1	0·284	0·0633	95,229	0·11	1·1	100 fibrous.
Soft cast steel	..	..	..	55·42	..	..	..	..	2·0	..
Hard cast steel	0·298	0·0697	10,967	157,346 = 70·2	0·289	0·0656	167,179	0·12	1·2	100 granular.
Soft Bessemer steel	0·297	0·0693	9,851	142,150 = 63·4	0·275	0·0594	165,841	0·16	1·6	100 granular.
Tungsten steel	0·300	0·0700	12,561	179,443 = 80·1	0·270	0·0570	220,368	0·73	7·3	{ 10 silky. 90 granular.

Presents, January 22, 1891.

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January 29, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Bakerian Lecture was delivered as follows:—

BAKERIAN LECTURE.—“On Tidal Prediction.” By G. H. DARWIN, F.R.S., Plumian Professor and Fellow of Trinity College, Cambridge. Received December 16, 1890.

(Abstract.)

At most places in the North Atlantic the prediction of high and low water is fairly easy, because there is hardly any diurnal tide. This abnormality makes it sufficient to have a table of the mean fortnightly inequality in the height and interval after lunar transit, supplemented by tables of corrections for the declinations and parallaxes of the disturbing bodies. But when there is a large diurnal inequality, as is commonly the case in other seas, the heights and intervals, after the upper and lower lunar transits, are widely different; the two halves of each lunation differ much in their characters, and the season of the year has great influence. Thus simple tables, such as are applicable in the absence of diurnal tide, are of no avail.

The tidal information supplied by the Admiralty for such places consists of rough means of the rise and interval at spring and neap, modified by the important warning that the tide is affected by diurnal inequality. Information of this kind affords scarcely any indication of the time and height of high and low water on any given day, and must, I should think, be almost useless.

This is the present state of affairs at many ports of some importance, but at others a specially constructed tide-table for each day of each year is published in advance. A special tide-table is clearly the best sort of information for the sailor, but the heavy expense of prediction and publication is rarely incurred except at ports of first-rate commercial importance.

There is not, to my knowledge, any arithmetical method in use of computing a special tide table which does not involve much work and expense. The admirable tide-predicting instrument of the Indian

Government renders the prediction comparatively cheap, yet the instrument can hardly be deemed available for the whole world, and the cost of publication is so considerable that the instrument cannot, or at least will not, be used for many ports at remote places. It is not impossible, too, that national pride may deter the naval authorities of other nations from sending to London for their predictions, although the instrument may, I believe, be used on the payment of certain fees.

The object, then, of the present paper is to show how a general tide table, applicable for all time, may be given in such a form that any one with an elementary knowledge of the Nautical Almanac may, in a few minutes, compute two or three tides for the days on which they are required. The tables are also such that a special tide-table for any year may be computed with comparatively little trouble.

Any tide-table necessarily depends on the tidal constants of the particular port for which it is designed, and it is supposed in the paper that the constants are given in the harmonic system, and are derived from the reduction of tidal observations. Where the observation has been by tide-gauge, the process of reduction is that explained in the Report to the British Association for 1883, but where the observations are only taken at high and low water, a different process becomes necessary. I have given in a previous paper a scheme of reduction in these cases.*

At ports not of first-rate commercial importance observation has rarely been by tide-gauge, and thus it is exactly at those ports, where the method of this paper may prove most useful, that we are deprived of the ordinary method of harmonic analysis. On this account I regard the previous paper as preliminary to the present one, although the two are logically independent of one another.

In the harmonic method the complete expression for the height of water at any time consists of a number of terms, each of which involves some or all of the mean longitudes of moon, sun, lunar and solar perigees; there are also certain corrections, depending on the longitude of the moon's node. The variability of the height of water depends principally on the mean longitudes of the moon and the sun and to a subordinate degree on the longitude of lunar perigee and node, for the solar perigee is sensibly fixed. There are, therefore, two principal variables, and two subordinate ones. This statement suggests the construction of a table of double entry for the variability of tide due to the principal variables, and of correctional tables for the subordinate ones; and this is the plan developed in the paper.

The mean longitudes of the moon and sun are not, however, convenient as variables, and accordingly the principal variables in the

* 'Roy. Soc. Proc.' 1890, vol. 48, p. 278.

tables are the time of moon's transit and the time of year; whilst the subordinate variables are the moon's parallax and the longitude of her node.

The tide-table, then, consists of the interval after moon's transit and height of high and low water, together with nodal and parallactic corrections, computed for every 20^m of moon's transit, and for about every ten days in the year. Each table serves for the two times of year at which the sun's longitude differs by 180°, and they may be used without interpolation. The nodal correctional terms consist of two times and of two heights, which are to be multiplied by the cosine and sine of the longitude of the moon's node, to give the total nodal corrections to the interval and height. The parallactic correctional terms consist of a time and a height, which are to be multiplied by the excess above, or defect below 57' of the moon's parallax at moon's transit to give the total parallactic corrections to the interval and height.

I had hoped that less elaborate tables might have sufficed, but it appeared that, at a station with very large diurnal inequality, the changes during the lunation, and with the time of year, in the interval and height are so abrupt and so great, that short tables would give very inaccurate results, unless used with elaborate interpolations. It is out of the question to suppose that a ship's captain would or could carry out these interpolations, and it is therefore proposed to throw the whole of that work on to the computer of the table.

Such a paper as this can only be deemed complete when an example has been worked out to test the accuracy of the tidal prediction, and when rules for the arithmetical processes have been drawn up, forming a complete code of instructions to the computer.

The port of Aden was chosen for the example, because its tides are more complex and apparently irregular than those of any other place which, as far as I know, has been thoroughly treated.

The arithmetic of the example was long, and was re-arranged many times. An ordinary computer is said to work best when he is ignorant of the meaning of his work, but in this kind of tentative work a satisfactory arrangement cannot be attained without a full comprehension of the reason of the method. I was therefore fortunate in securing the enthusiastic assistance of Mr. J. W. F. Allnutt, and I owe him my warm thanks for the laborious computations he has carried out. After computing fully half the original table, he made a comparison for the whole of 1889 of our predictions with those of the Indian Government. Without going into the details of this comparison, it may be mentioned that the probable error of the discrepancy between the two tables was 9^m in time, and 1·2 inches in the height of high water; that there were reasons to expect some systematic difference between the two calculations, and that all the considerable

errors of time fall on those very small rises of water which are of frequent occurrence at Aden.

I have made two other comparisons, one with the Indian predictions of 1887, and the other with actuality of 1884. In the latter case, when a few very small tides were omitted, the probable error was 7^m in the time, and 1.4 inches in height. It is concluded from these comparisons that, with good values for the tidal constants, the tables lead to excellent predictions, even better than are required for nautical purposes.

It is probable that this method may be applied to ports of second-rate importance, where there are not sufficient data for very accurate determination of the tidal constants. Suggestions are made for very large abridgment of the tables in such cases, accompanied, of course, by loss of accuracy.

The question of how far to go in each case must depend on a variety of circumstances. The most important consideration is, I fear, likely to be the amount of money which can be expended on computation and printing; and after this will come the trustworthiness of the tidal constants, and the degree of desirability of an accurate tide-table. The aim of the paper has been to give the tables in a simple form, and if, as seems certain, the mathematical capacity of an ordinary ship's captain will suffice for the use of the tables, whether in full or abridged, I have attained the principal object in view.

Presents, January 29, 1891.

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The Academy.

February 5, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Right Hon. William Lawies Jackson, whose certificate had been suspended, as required by the Statutes, was balloted for and elected a Fellow of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

- I. "On the Chief Line in the Spectrum of the Nebulæ." By J. NORMAN LOCKYER, F.R.S. Received December 18, 1890.

[Publication deferred.]

- II. "On the Chief Line in the Spectrum of the Nebulæ. A Reply." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. Received February 5, 1891.

[Publication deferred.]

III. "On a Membrane lining the Fossa Patellaris of the Corpus Vitreum." By T. P. ANDERSON STUART, M.D., Professor of Physiology in the University of Sydney, N.S.W. Communicated by Professor SCHÄFER, F.R.S. Received January 12, 1891.

The 9th edition of Quain's 'Anatomy,' 1882, after giving a description of the hyaloid membrane and its connexions, says, "According to the account usually given, the hyaloid membrane divides in front into two layers: an anterior, continued forwards as the zonule of Zinn, and a posterior, passing behind the lens, the canal of Petit being contained between them. The above description is based upon a renewed original investigation into the relations of the structures which support the lens, and is confirmatory of the statements of Meckel, Henle, Brailey, and others, and opposed to those of Iwanoff." Now the description adopted by Quain discards the posterior layer passing behind the lens. The vitreous humour, according to it, lies immediately against the posterior layer of the lens capsule, and at the canal of Petit may, perhaps, in part occupy the interstices of the suspensory fibres, which are said to pass from the zonula to the periphery of the lens capsule. Thus the whole anterior surface of the vitreous is bare, that is, is not invested by any membrane.

I cannot agree with this view, for, in the eye of the ox, I have demonstrated to the satisfaction of large numbers of my students and many members of the medical profession in Sydney, and at the Inter-colonial Medical Congress in Melbourne, 1889, that there is undoubtedly a membrane in this situation. I have found it likewise in the eye of the sheep, goat, dog, and porpoise, so that I entertain no doubt of its general occurrence, notwithstanding that Schwalbe, in 1887 ('Anatomie der Sinnesorgane'), adheres to the view of the non-existence of the membrane. This view was more explicitly set forth by Schwalbe in the anatomical part of De Wecker and Landolt's 'Traité complet d'Ophthalmologie' (Paris, 1886). Here, at p. 519, vol. 11, he says what I translate as follows:—

"In the region of the ora serrata the hyaloid begins to gradually thicken and to change its structure, becoming the zonula ciliaris. From this point it constitutes the anterior wall of the canal of Petit; the posterior wall is identical with the anterior surface of the jelly of the vitreous body, which is differentiated from the liquid contents of the canal of Petit merely by its more dense surface. A cleavage of the zonula, near the ora serrata, into an outer leaflet representing the fibres of the zonula and an inner one lining the fossa patellaris does not take place. Consequently the canal of Petit is to be compared to the other clefts in the jelly of the vitreous body. This description

differs from those formerly given by me, in that I now consider the membrane limiting the canal of Petit posteriorly as an artificial product, the result of the action of reagents (by precipitation or condensation). There I agree with Iwanoff. I am bound, however, to maintain, in opposition to Meckel, the existence of the canal of Petit, such as I have described it; that the zonula and the vitreous body appear to touch each other is but the result of the compression which they undergo during section. And so a cleavage of the hyaloid near the ora serrata does not take place." Iwanoff says, "In the vicinity of the ciliary processes the vitreous separates itself from the zonula, so that its entire anterior surface, or that which is turned towards the canal of Petit and the lens, is not covered by any special membrane; neither by a prolongation of the limitans, as stated by Henle, nor by a special membrana hyaloidea, as was formerly supposed." Then, "At the ora serrata the several concentric layers of the cortex (of the vitreous) are so crowded together that the surface of the nucleus is separated from the limitans only by a very thin but plainly fibrous layer. The fibres of this layer run parallel to the surface of the vitreous in wavy bundles, and are not unlike bundles of connective tissues. The entire layer, thus changed, finally turns and passes towards the axis of the eye, thus completely covering the anterior surface of the vitreous. Since we here, in fact, have not a single but several layers crowded together, and only loosely united with one another, it is easy to see how one might suppose that behind the lens there lay a special membrane covering the corpus vitreum, especially since the most superficial of these layers is perfectly smooth." Finally, "The tissue of the vitreous is here condensed to form a limiting layer, in the same manner as Bowman's membrane is formed by a condensation of the substantia propria of the cornea; an independent membrane—the hyaloidea—does not exist at this place."

What occurs to me then, considering the eminence of the authorities on each side of the question, is that the methods of demonstration have not been sufficiently conclusive. After seeing it as I have seen it, and shown it to others, I cannot for one moment doubt its existence, for the proof of its existence could not possibly be more conclusive—it can even be dissected off and examined in any perfectly fresh unaltered ox eye.

As Aeby has already, I find, published, the eyeball is best left to decompose for some twenty-four hours or longer, according to the external temperature, and then, on opening the sclerotic and choroid tissues carefully with fine blunt-pointed scissors, the vitreous and lens, united by the suspensory ligament, drop out in a mass, or at least are very easily expressed. The suspensory ligament is now snipped all round, and the lens in its capsule removed.

When this has been done, according to the one side, the bed of the

lens—the fossa patellaris—and the posterior wall of the canal of Petit (now opened up) would be bounded or lined immediately by the substance of the corpus vitreum. According to the other side—with which I entirely agree—there stretches from side to side a distinct membrane, so that in no part of its extent does the substance of the corpus vitreum reach the surface. Let me now proceed to the proofs, which are of various kinds—chemical, optical, and mechanical.

Chemical Pigments.—Aniline dyes and picrocarmine stain the capsule of the lens, the hyaloid, and other such elastic membranes. When the fresh corpus vitreum is so stained—and I prefer strong picrocarmine for some three minutes, then washing in copious water—the hyaloid is perfectly well seen floating in water, with its wrinkles on distortion and its well-defined free edge at a puncture. Exactly the same appearance is seen on the front of the corpus vitreum—here there is something that stains deeply and that wrinkles. Moreover picrocarmine stains the hyaloid membrane and the vitreous substance differently: the former is red and the latter is yellow. The same difference is seen at the edge of a puncture in the floor of the patellar fossa: the red membrane is quite distinct from the yellow vitreous substance.

Optical.—If, by means of a lens, the sun's rays be concentrated upon the hyaloid membrane, it is seen to have a fluorescent appearance, somewhat as if the surface had been bathed in a solution of quinine sulphate. That this fluorescent appearance is due to the hyaloid is obvious when the concentrated rays are made to fall on a puncture in the hyaloid membrane. The vitreous substance itself has no such appearance, but is clear and glassy, so that the puncture is beautifully seen, and the edge of the hole is sharp and well defined. Exactly the same appearances are obtained when we examine the front of the vitreous; the fluorescence is here too, and the difference between this appearance and that of the vitreous substance showing through the puncture is very marked.

Mechanical.—When a blunt-pointed instrument is gently pressed upon the hyaloid membrane and then removed, the substance recoils simply, perhaps leaving a dimple for a little time; but, on pressing more firmly, there comes an instant when the instrument suddenly sinks; one has the impression that a membrane has been punctured, and that behind the membrane the substance is soft and inelastic. This impression is at once supported on squeezing the mass of the vitreous between the fingers; a little elevation of the vitreous substance is projected like a pimple through the opening in the membrane, and recedes when the pressure is withdrawn. When this is repeated in the front of the vitreous, the results are identical.

So far, I have not mentioned anything which might not be equally well explained by supposing the existence of a dense superficial layer

of the vitreous substance, but the membrane is no such thing; it is a true membrane; it can readily be isolated, stained, submitted to microscopical examination, &c.

Even with the unstained vitreous, it is quite easy to introduce a blunt instrument through a puncture in the membrane and, by working the instrument about under the surface, to detach the membrane from the surface of the vitreous substance.

When this has been done, a bell of air blown under it displays the membrane to good advantage as a delicate, elastic, smooth, apparently structureless, perfectly transparent sheet of tissue, answering most completely to the term "hyaloid." Though delicate, it is yet strong enough to support the whole weight of the vitreous when a blunt instrument is put into it.

When, the bounding membrane remaining intact, the vitreous is squeezed so as to bulge its anterior face, that face does not bulge equally all over its extent. The centre of the anterior face projects more than the peripheral ring. The central projected part corresponds to the fossa patellaris, where, as I shall show, the patellar membrane is thin, while the peripheral ring forms the back wall of the canal of Petit, and here the membrane is comparatively thick. The transition from the peripheral to the central parts is fairly sudden, for the central elevation rises from a distinct line corresponding to the inner margin of the peripheral ring. The canal of Petit is, therefore, a true canal.

If the vitreous be inverted over the mouth of a test-tube (with a hole in the bottom of it) of about $\frac{1}{2}$ inch diameter, and tied over it with a thick silk thread, and afterwards with a rubber band, the superficial part of the hyaloid and greater mass of the vitreous is cut through. If now the vitreous substance be carefully pulled off by forceps, or if the test-tube be set upright in a beaker, and water poured into the beaker, the water rising in the tube will bulge the membrane so that the vitreous substance will drain off it in an hour or so. The membrane thus isolated is toughened by exposure over night, so that such a membrane, though it looks like a mere film, yet sustained no less a pressure than 40 inches of water; others sustained 22, 28, 34 inches, and so on, even after having been dead for days.

If the membrane be snipped all round its periphery, it can be detached as a whole from the subjacent vitreous substance.

When it has been removed, little tags of deeply-staining material are sometimes seen projecting from its deep face; these, I have thought might be vestiges of the hyaloid artery; but, whether these are there or not, there is little or no adhesion between the membrane and the vitreous substance.

When removed, and its deep surface brushed under water to remove any adherent vitreous substance, it is seen to be a hyaloid

membrane with a thin centre and thick periphery. Under the microscope it is structureless. On removal it, of course, stains deeply, and thus can be readily examined.

When one attempts to raise it outwards towards the hyaloid membrane and suspensory ligament, one may succeed as far as the origin of the suspensory ligament, but behind this point it is so firmly adherent to the vitreous substance that it cannot be raised.

The notion of a membrane in front of the vitreous is supported by the behaviour of the vitreous body with its investing membranes intact in water; it will remain many days with its form quite unchanged, and during all this time it may be handled without injuring it. But if the membranes be cut so as to expose the vitreous substance to the action of the water, this substance protrudes and has a cloud-like outline very different from the sharp, definite outline or surface at the uninjured anterior face of the vitreous body where still covered by membrane. Now there is never any of this cloud-like indefinite outline or surface at the uninjured anterior face of the vitreous body. I infer, therefore, that it is not vitreous substance that here comes into contact with the water, but that it is a membrane that is not notably acted on by water.

After all these facts and considerations, I cannot doubt that there is in the perfectly fresh unaltered eye a membranous structure behind the posterior layer of the lens capsule, and that this structure has all the properties of a distinct membrane resembling the hyaloid, but differing in many respects from vitreous substance.

I need say nothing here as to the immense importance in many questions of ophthalmological practice of a definite knowledge of the existence or non-existence of a membrane limiting the vitreous body anteriorly.

[*Note added January 15, 1891.*—Since the above was sent in, I have had an opportunity of examining a series of sections of the entire human eyeball, made by Dr. Sheridan Delépine, and in all of these sections the membrane is distinctly seen *in situ*.]

IV. "On the Connexion between the Suspensory Ligament of the Crystalline Lens and the Lens Capsule." By T. P. ANDERSON STUART, M.D., Professor of Physiology in the University of Sydney, N.S.W. Communicated by Professor SCHÄFER, F.R.S. Received January 12, 1891.

I have not been able to get a too precise statement as to the nature of this connexion, but Quain (9th ed.) says the suspensory ligament is "firmly attached" to the capsule; in another place Quain says it "joins" it. Speaking of "suspensory fibres of the lens," Quain says

that some of these "pass into continuity with the posterior capsule." Thus "attachment," "joining," and "passing into continuity" are the expressions used to indicate the connexion. It is true that the last is employed with regard to the suspensory fibres, but since these, as described, are, like the suspensory ligament, derived from the hyaloid membrane and pass like it to the lens capsule, I think we may assume that the author in "Quain" regards them—fibres and ligament—as of like nature and mode of union with the lens capsule.

Schwalbe ('Anatomie der Sinnesorgane') says the capsule is firmly united (*verwachsen*) with the zonula. Later, he speaks of the outer or zonular layer of the lens capsule being joined (*in Verbindung*) to the zonula; then again of its firm connexion (*fester Zusammenhang*) with the zonula when he uses this intimate union as an argument in favour of the zonular layer of the capsule being of connective tissue origin. In describing the zonula he says that its parts fuse (*verschmelzen*) with the capsule without any perceptible line of demarcation, and probably form the above-mentioned zonular layer. Finally, the mode of fusion is as follows: The coarser bundles break up into a network of finer fibrils, which spread out on the surface of the capsule and, becoming pointed, lose themselves (*sich verlieren*) in the substance of the capsule.

From the various statements, I think it is clear that the general notion is that there is a direct continuity of substance between the suspensory ligament and the capsule. Now the observation which I am about to describe seems rather to indicate that the suspensory ligament is *only cemented to the capsule*.

Upon opening some ox eyes that were in an advanced state of decomposition, I found that the lens was quite free in the interior of the eyeball; and, on examining it, I found that it was still enclosed in its capsule. This freeing of the lens I find to be the rule in such cases. On opening the capsule, the lens substance escaped, and on washing and staining the capsule with picrocarmine and other dyes, and on examining it in various ways, I have failed to find any roughness of surface, difference of thickness, or, in short, any indication of a rupture of tissue. The zonula seems to come away intact: is not broken or torn away. In fact, the decomposition seems to weaken the cohesion of some cement substance by which the zonula adheres to the surface of the lens capsule.

This observation seems to weaken the *argument* for an outer layer of the capsule being of connective tissue origin, and it may throw some light on cases of solution and atrophy of the suspensory ligament, on cases of detachment of the ligament from its insertions, and on cases of luxation of the lens. In any case it has a very direct bearing on the still unsettled question of the development of the lens capsule.

V. "A simple Mode of Demonstrating how the Form of the Thorax is partly determined by Gravitation." By T. P. ANDERSON STUART, M.D., Professor of Physiology in the University of Sydney, N.S.W. Communicated by Professor SCHÄFER, F.R.S. Received January 12, 1891.

It is a well-known fact that the quadrupeds have the transverse section of the thorax elliptical with the long axis vertical. This form of thorax, more or less, is possessed also by the human foetus. As the erect posture is gradually assumed in the development of species and of the human individual the ventro-dorsal and transverse diameters approximate to each other, and then, the process continuing, in the adult the transverse diameter exceeds the antero-posterior.

That these are the forms proper to the thorax when under the influence of gravitation alone is seen by holding a hoop made of a strip of ordinary crinoline steel $\frac{1}{2}$ inch wide and about 6 feet long, so that its plane is vertical; its form is that of an ellipse. Now grasp the hoop firmly between the fore-finger and thumb of one hand, and gradually turn the internal face of the portion grasped till it looks straight forwards. The front part of the hoop will, of course, be lower, corresponding in some measure to the slope of the ribs, &c. At the same time the diameters approximate to each other. Continue the turning till the face that looked straight forwards looks upwards and forwards, so that in fact the plane of the grasped portion corresponds to that in which the lower dorsal region of the vertebral column of man lies. The slope of the ribs is lessened, but the interesting points are that the transverse diameter exceeds the antero-posterior, and the exact curve and direction of the surface of the lower ribs are reproduced. Then are seen the twist in the long axis of the rib and likewise that great hollow on each side of the vertebral column which is so marked a feature in the human thorax.

I do not overlook the fact that the conditions in the organism are not just the same as they are in this simple hoop; but I think it will be conceded that where there is a force so constant and so potent in its action as is that of gravitation it will be yielded to by the organism unless there be some good reason to the contrary. Now there does not seem to me to be any such reason here, and it is interesting to observe how closely the thorax of the animal follows the lines of the hoop of steel when the conditions as to gravitation are the same.

I am thus led to suspect that gravitation has had a larger share than is usually thought in moulding the form of the vertebrate thorax both in health and disease.

Any strip of elastic material will do for the above if the length be suitable—one readily finds the proper length by trying larger and smaller circles.

VI. "On the Physiology of Asphyxia, and on the Anæsthetic Action of Pure Nitrogen." By GEORGE JOHNSON, M.D., F.R.S. Received January 26, 1891.

(Abstract.)

The main object of this paper is to bring forward additional evidence in support of the theory that the immediate cause of death in cases of asphyxia is the arrest of the pulmonary circulation. I have to express my obligation to my friend Mr. Charles James Martin, M.B., B.Sc., Demonstrator of Physiology in King's College, for the time and labour which, by my request, he has bestowed in the performance of numerous and various experiments, the results of which will, I think, throw much light upon the complex phenomena of asphyxia. It is right to mention that Mr. Martin is not responsible for my interpretation of the results of his experiments.

All the experiments were performed on animals under the influence of anæsthetics, and every animal was finally killed by deprivation of air.

Animals—rabbits, cats, and, in a few cases, dogs—were asphyxiated either by ligature of the trachea, by the paralysing influence of curara, or by causing them to inhale a gas containing no free oxygen, viz., nitrous oxide, pure nitrogen, hydrogen, and carbonic acid gas. In all these experiments, re-inspiration of the gases was avoided by allowing the expired gas to escape through a T-tube fixed in the trachea.

During the performance of the experiments, in most cases, the chest and pericardium of the animals were opened so that the relative fulness of the cavities might be readily observed. In all the experiments, the results, as regards distension of the heart's cavities, were essentially the same, no matter whether the air was simply excluded or whether an azotic gas (*i.e.*, a gas, not in itself poisonous, but unable to support life) was substituted for atmospheric air; the only difference being that when an azotic gas is inhaled the phenomena are far more rapidly produced, in consequence of the more speedy displacement of oxygen from the lungs.

The principal changes in the heart's cavities were, first, distension of the left cavities; second, enormous distension of the right cavities with diminished distension of the left, the circulation being apparently arrested by the inability of the right cavities to empty themselves, in

consequence of obstruction in front. That the arrest of the circulation is not due to paralysis of the heart's walls, by the circulation of venous blood through its tissues, seems to be proved by the following experiment.

Into the trachea of a small dog, with the chest and pericardium opened and kept alive by artificial respiration, a glass T-tube was introduced, through which pure nitrous oxide was passed into the lungs, whilst the expired gases escaped into the air. As usual, first the left then the right cavities became distended, and in one minute the heart's action had nearly ceased, with over-distension of the right side. Then inhalation of nitrous oxide, *impregnated with the vapour of nitrite of amyl*, was substituted for pure N_2O , by means of a two-way stopcock, and the result was that almost immediately the distension of the right cavities began to subside, and in two minutes they had nearly regained their normal size.

The explanation is, that the circulation, having been arrested by the contraction of the arterioles, was, for a time, restored by the paralyzing influence of nitrite of amyl upon those vessels, while atmospheric air was strictly excluded.

Additional evidence of the influence of the arterioles in arresting the circulation during the progress of asphyxia is derived from the fact that a sufficient dose of such agents as are known to paralyse the arterioles, *e.g.*, curara and atropine, prevents over-distension of the heart's cavities, and considerably prolongs the life of the animal.

This is conclusively shown by experiments performed by Mr. Martin, the details of which are given in the paper of which this is an abstract.

It has been suggested that the distension first of the left then of the right side of the heart in asphyxia is the result solely of *systemic* arterial contraction, the impediment acting backwards from the left side of the heart, through the lungs, to the right cavities and the systemic veins. The main objection to this theory is the fact that, when the chest is opened immediately after death from asphyxia, the lungs are found extremely pale, from anæmia of their minute vessels, and in a corresponding degree collapsed. Backward pressure from the left side of the heart, sufficient to greatly distend the right cavities, must of necessity involve engorgement of the pulmonary capillaries.

That there is a certain amount of backward pressure from the primary distension of the left heart, extending as far as the pulmonary veins, would seem to be proved by observations made by Mr. Martin to the effect that a manometer in a branch of a pulmonary vein indicates an early and continuous increase of pressure during the progress of asphyxia; but that this backward pressure does not extend to the right side of the heart is shown by the fact that in the

last stage of asphyxia, while the right cavities are in a state of extreme distension, the left are, as a rule, flaccid and comparatively empty, the lungs themselves, as before mentioned, being extremely anæmic and collapsed. The condition of the heart's cavities in the successive stages of asphyxia was clearly shown by an experiment which Dr. Rutherford performed in my presence in 1873. The details of this experiment are given in my paper (see diagram with tracing).

The true explanation of these facts appears to be that, during the latter stages of asphyxia, the pulmonary arterioles contract, and cause the extreme distension of the right cavities with anæmia of the pulmonary capillaries, and a corresponding defective supply to the left cavities of the heart.

The continued increase of pressure in the pulmonary vein, observed by Mr. Martin, may perhaps be accounted for by the fact that in the last stage of asphyxia the suction power of the left auricle is impaired, partly by anæmia of the cardiac tissue, consequent on the contraction of the arterioles—both pulmonary and systemic, the coronary included—and partly by the fact that the small amount of blood with which it is supplied is more or less completely de-oxygenated.

[I venture further to suggest the following explanation of the increased blood pressure which has been observed to occur in the pulmonary veins during the successive stages of asphyxia. During the first stage, when the left cavities of the heart are over-distended, as seen in Dr. Rutherford's experiment, there would be a backward pressure extending through the pulmonary veins and capillaries, even, perhaps, to the branches of the pulmonary artery; but this backward pressure from the left side of the heart must obviously cease when, in the last stage of asphyxia, those cavities are nearly empty of blood. When, however, portions of the ribs are removed in order to introduce a manometer into one of the pulmonary veins, a new and artificial cause of obstruction to the pulmonary venous circulation is introduced.

The collapse of the lung, which results from the breach in the chest wall, compresses the thin-walled pulmonary veins more than the corresponding arteries, and so increases the intra-venous pulmonary pressure. It is an acknowledged fact that the comparatively slight compression of the pulmonary veins which occurs towards the end of a normal expiration lessens the flow of blood into the left side of the heart.* It is obvious, however, that the pulmonary venous obstruction thus caused must be very much less than that occasioned by the extreme collapse of the lung which results from an opening in the wall of the chest.—March 3, 1891.]

* See Dr. M. Foster's 'Physiology,' 5th edition, p. 618.

Drs. Bradford and Dean have proved not only the existence of pulmonary vaso-motor nerves, but also that they leave the cord higher up than the systemic vaso-motor nerves (*vide* 'Roy. Soc. Proc.,' vol. 45).

These authors remark that "it is probable that the pulmonary vaso-motor mechanism is but poorly developed, compared with that regulating the systemic arteries."

It would indeed be an incredible physiological anomaly if the vessels of an organ, through which the entire blood of the body has constantly to pass, had not the same regulating and resisting power, compared with the force of the right ventricle, as that possessed by the systemic arterioles.

Mr. Martin has found by introducing a manometer into a branch of the pulmonary artery of a moderate sized cat, while the remaining branches were suddenly obliterated, that the blood pressure was rather more than doubled, rising from 17 mm. to 36 mm. of mercury.

Mr. Martin also found that, during the last stages of asphyxia, the pressure in the pulmonary artery is nearly doubled, while that in the carotid is rapidly falling.

No experiment that has hitherto been devised can accurately measure the resisting power of the pulmonary arterioles or the actual force of the right ventricle, for the reason that the arrest or great diminution of the pulmonary circulation weakens the muscular walls of the heart by cutting off the blood supply through the coronary arteries.*

The increase of systemic arterial blood pressure, which instantaneously follows re-admission of air into the lungs, after the circulation had been almost completely arrested by exclusion of air, seems to prove that the heart's walls are not *paralysed* by venous blood. On the other hand, such a speedy restoration of the circulation is at once explained by the sudden removal of the obstruction which had been caused by the contracted pulmonary arterioles.

* [Since this paper was communicated to the Royal Society, Dr. M. Foster has done me the favour to refer me to a paper by Professor Knoll ("Der Blutdruck in der Arteria pulmonalis," 'Sitzber. Akad. Wiss. zu Wien,' vol. 97, Abth. 3, p. 207). Dr. Knoll endeavours to measure the normal blood pressure in the pulmonary artery of the rabbit by dividing the sternum, opening the pericardium, and introducing a tube into the pulmonary artery without wounding the pleura. Thus, the blood pressure is observed while normal respiration is carried on.

Dr. Knoll, however, admits that the atmospheric pressure, consequent on the opening of the mediastinum, cannot be without some influence upon the circulation, so that even this careful and difficult mode of procedure is not free from sources of error.—March 3, 1891.]

Conclusions relating to Asphyxia.

That the immediate cause of death is the arrest of the pulmonary circulation appears to be proved by the following facts:—

1. When the chest of an animal is opened immediately after death caused by a ligature on the trachea, the right cavities of the heart are found enormously distended, while the left are comparatively empty.

2. When the heart of an animal is exposed during the progress of asphyxia, the right cavities are seen to become distended, while the left, which had been previously gorged, are found to be collapsed and nearly empty.

3. In the last stage of asphyxia there is a continuous increase of pressure in the pulmonary artery while the systemic arterial pressure is falling.

4. That the arrest of the circulation through the lungs is due to the contraction of the pulmonary arterioles appears to be proved by the influence of agents which paralyse the arterioles, namely, nitrite of amyl, atropine, and an excessive dose of curara; the effect of which is that deprivation of air is unattended by distension of the right cavities of the heart and other evidence of obstructed pulmonary circulation, the life of the animal is prolonged for several minutes, and death ultimately results from the influence of venous blood upon the cardiac and nervous tissues.

The anæsthetic action of nitrogen alone or with a small proportion of oxygen. The phenomena which result from the inhalation of nitrous oxide as an anæsthetic are strictly analogous with those observed in the early stages of asphyxia.*

Some writers maintain that the anæsthetic action of nitrous oxide is due to its preventing access of free oxygen to the system, others believe that it has a specific anæsthetic action. It occurred to me that light might be thrown upon this subject by the administration of pure nitrogen. Accordingly I obtained from the Scotch and Irish Oxygen Company, of Glasgow, a cylinder containing 100 cubic feet of compressed nitrogen in which the proportion of oxygen was only 0·5 per cent. by vol., whilst that of the CO₂ present was 0·3 per cent. As a preliminary trial, Mr. F. W. Braine was good enough to administer this gas in five instances to members of the staff of King's College, who volunteered to inhale it.

The result was, in each case, the production of complete anæsthesia and of general phenomena precisely similar to those observed from the inhalation of nitrous oxide. Encouraged by these results, Mr.

* *Vide* the author's 'Essay on Asphyxia,' p. 30.

Braine felt justified in administering the gas to patients at the Dental Hospital. Nine patients took the gas. In every case, the result was the production of complete anæsthesia, with general phenomena precisely similar to those observed during nitrous oxide inhalation. The pulse was first full and throbbing, then feeble; in the advanced stage respiration was deep and rapid, with lividity of the surface, dilated pupils, and more or less jactitation of the limbs; the only difference, in the opinion of some of those present, being that the anæsthesia was less rapidly produced, and somewhat less durable than that from nitrous oxide, though in each case the tooth was extracted without pain.

On a subsequent occasion, the same gas was administered by Dr. Frederic Hewitt at the Dental Hospital. Nine patients took the gas. The maximum period required to produce anæsthesia was 70 seconds, the minimum 50 seconds, and the mean time 58·3 seconds.

In one case, two teeth were extracted without pain; in one only was pain experienced, and in that case, the tooth having been broken and not extracted, the patient said she felt a "smashing up."

I subsequently obtained from the same Company a cylinder containing compressed nitrogen with 3 *per cent. of oxygen*, and a second cylinder containing nitrogen with 5 *per cent. of oxygen*. These gases were also administered by Dr. Hewitt to patients at the Dental Hospital, with the following results.

Five patients took the 3 *per cent. gas*. Anæsthesia was complete in 75 seconds (max.), and in 60 seconds (min.), the average time required being 67·5 seconds. In each case, the tooth was extracted without pain, the duration of anæsthesia being somewhat longer than with pure nitrogen. In each case there was lividity, dilatation of pupils, and more or less jactitation.

Four patients took the nitrogen containing 5 *per cent. of oxygen*. With this mixture, the time required for the production of anæsthesia ranged from 75 to 95 seconds, the average time being 87·5 seconds.

In each case there was complete anæsthesia, during which one patient had three molars extracted. Although she said she felt the last two, the sensation appeared to be that of a pull, and not of acute pain.

In most of these four cases there was slight lividity before the removal of the face-piece. In only one case was there slight jactitation of the limbs; the other three patients were perfectly quiescent.

For the information of those who may be disposed to investigate the anæsthetic action of nitrogen with a small proportion of oxygen, I may mention that Brin's Oxygen Company (69, Horseferry Road, Westminster) are prepared to supply nitrogen containing from 4 to 7 *per cent. of oxygen* at the same rate as they charge for pure oxygen.

Below 4 per cent. of oxygen nitrogen could be supplied only by special arrangement and probably at increased cost.

Presents, February 5, 1891.

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February 12, 1891.

Professor ALFRED NEWTON, M.A., Vice-President, in the Chair.

The Right Hon. William Lawies Jackson was admitted into the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

- I. "On the Organisation of the Fossil Plants of the Coal-measures. Part XVIII." By W. C. WILLIAMSON, LL.D., F.R.S., Professor of Botany in the Owens College, Manchester. Received January 22, 1891.

(Abstract.)

On three preceding occasions the author has directed attention to the existence in the older Carboniferous rocks of a remarkable form of fructification which seemed to belong to the Calamarian family of plants, though presenting features distinct from any that had hitherto been described. In the first instance, in 1871, he placed this fructification in Sternberg's provisional genus *Volkmannia*, under the name of *V. Dawsoni*. Some small fragments of the same type, obtained at a later period by the late Professor Weiss, of Berlin, led him to identify the plant with Binney's hitherto very obscure genus *Bowmanites*, an identification which is accepted by Professor Williamson. Still more recently, a number of additional specimens have been obtained from the Ganister Carboniferous beds of Lancashire and Yorkshire, which not only throw further light upon the plant, but have made it possible to re-write its history in an almost complete form.

Like all the other Calamariæ, *Bowmanites* was a plant with a distinctly articulated stem, each node of which bore a verticil of lateral appendages. In the vegetative organs each of these nodal appendages consisted of a verticil of the linear, uninerved leaves characteristic of the old, ill-defined genus *Asterophyllites*. In the fructification these foliar verticils are replaced by a broad circular disk, the margin of which sustained a verticil of leaf-like "disk-rays." These rays can scarcely, at present, be identified with true leaves, since they have

not only no midrib, but they seem to contain no traces whatever of a vascular bundle.

The centre of the axis of the strobilus is occupied by a conspicuous bundle of barred and reticulated tracheids of the scalariform type, the transverse section of which bundle is triangular, with concave sides. Each of the three prominent angles is abruptly and broadly truncated. A thin inner cortex seems to have originally surrounded this bundle, but all traces of its tissues have disappeared. The thick outer cortex is composed of a mixture of rather coarse, strongly defined parenchymatous and prosenchymatous cells. At each node this cortex expands into the lenticular disk already referred to. This disk is thickest at its inner border, thinning gradually towards its outer margin, where it subdivides into the verticil of elongated disk-rays already mentioned. Though no vascular bundles can be discovered connecting the central axial one with the surrounding disk, some such must have once existed, since we find them both in the cortex of the internodes and in the nodal disks.

The entire upper surface of each disk has given off numerous very slender sporangiophores, destined to reach three or four concentric circles of sporangia, which were arranged in a single plane in the internodal interval between each two disks. Each sporangiophore, unlike what is usual amongst the *Calamariæ*, only sustained a single sporangium. In order to reach the more external ranges of the latter organs, the sporangiophores were prolonged outwards in a distinct layer between the upper surface of the disk and the sporangia which rested upon it. Not only was this the case, but when each sporangiophore reached the sporangium with which it was destined to become organically united, it did not at once do so; but it passed under, and even beyond that organ, when it bent back upon itself and became united to the sporangium on its distal side. The outer, or epidermal, layer of the sporangium was merely an extension of that of the sporangiophore.

The numerous spores of *Bowmanites* have also a distinctive form. Each has a rather thin exosporium, but this is thickened along a few reticulate lines, and from each junction of these reticulations a strong radiating spine is projected. It is in the very distinctive features of these reproductive organs that the marked generic individuality of *Bowmanites* chiefly resides.

The second plant described in the memoir, under the name of *Rachiopteris ramosa*, is one of the several Fern-like organisms which the author has included in his provisional group of *Rachiopterides*. Considerable doubt exists respecting the true affinities of at least some of these plants. The one now described may prove to be a less hirsute, more fully developed condition of the *Rachiopteris hirsuta* described by the author in his Memoir XV.

II. "On Certain Ternary Alloys. Part III. Alloys of Bismuth, Zinc, and Tin, and of Bismuth, Zinc, and Silver." By C. R. ALDER WRIGHT, D.Sc., F.R.S., Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School, and C. THOMPSON, F.I.C., F.C.S. Received January 24, 1891.

The general methods adopted in carrying out the experiments detailed below were identical with those described in Part II,* the weighed metals being fused together under cyanide of potassium, well intermixed by continued vigorous stirring, and poured into red-hot narrow clay test-tubes, which were then inserted inside thin iron protecting tubes, closed at the lower end, and immersed in a bath of molten lead, maintained at as nearly as possible a constant temperature by means of a series of Bunsen flames playing into the interspace between the cylindrical iron vessel containing the lead and an outer concentric clay jacket. Fluctuations of temperature, to a greater or lesser extent, being unavoidable during long periods of heating, notwithstanding all care, some of the figures ultimately deduced were less concordant than those obtained in the experiments previously described, the mode of separation of a given mass into two different alloys being apparently more affected by temperature variations with mixtures containing bismuth than with corresponding mixtures containing lead instead of bismuth.

Our first experiments were made with the object of determining the mutual solubilities of bismuth and zinc at different temperatures. Matthiessen and v. Bose found ('Roy. Soc. Proc.,' vol. 11, p. 430) that melted bismuth could dissolve zinc, forming an alloy containing 12.93 to 16.30 per cent. of zinc (excluding two determinations of 8.65 and 8.80 per cent. respectively, presumably erroneous from some undetected source of error); and that zinc could dissolve bismuth, forming an alloy containing 2.38 to 2.48 per cent. of bismuth; no record of temperature was made in these observations further than that the molten metals were ultimately poured into "a porous cell which had been previously heated to redness in a large crucible filled with sand."

We obtained the following figures, approximately equal weights of bismuth and zinc being melted together and well stirred up, the mixtures being then poured into red-hot narrow clay test-tubes, and heated in the lead-bath for periods of eight hours and upwards.

* 'Roy. Soc. Soc.,' vol. 43, p. 25; Part I, *ibid.*, vol. 45. v. 461.

Zinc dissolved by Bismuth.

Temperature range	550—650°.	650—750°.	750—850°.
Percentage of zinc in alloy.....	14·03	14·42	15·27
„ „ „	14·05	14·62	15·59
„ „ „	—	—	16·19
„ „ „	—	—	16·26
Average.....	14·04	14·52	15·83

Bismuth dissolved by Zinc.

Percentage of bismuth in alloy...	2·20	2·24	2·44
„ „ „	2·24	2·60	2·60
Average	2·22	2·42	2·52

From these figures it results that the following percentages of zinc and bismuth respectively are dissolved for the mean temperatures 650°, 750°, and 800°.

Zinc dissolved by Bismuth.

Range	550—650°	14·04	Range 650—750°	14·52	Range 750—850°
„	650—750°	14·52	„ 750—850°	15·83	
Approximate mean temp. of 650°.....	} Average ..	14·28	Mean temp. of 750°.....	} 15·18	Mean temp. of 800°
					} 15·83

Bismuth dissolved by Zinc.

Range	550—650°	2·22	Range 650—750°	2·42	Range 750—850°
„	650—750°	2·42	„ 750—850°	2·52	
Approximate mean temp. of 650°.....	} Average ..	2·32	Mean temp. of 750°.....	} 2·47	Mean temp. of 800°
					} 2·52

Hence, while the solubility of bismuth in zinc increases between 650° and 800° to an extent only just measurable and barely outside the limits of experimental error, that of zinc in bismuth increases

in a much more marked fashion. The formula Zn_4Bi_7 requires $\text{Zn} = 15.1$, and $\text{Bi} = 84.9$ per cent.

Mixtures of Bismuth, Zinc, and Tin.

Two series of experiments were made, one at a temperature ranging between 600° and 700° , and averaging near to 650° ; the other ranging between 700° and 800° , and averaging near to 750° . The results indicate that in presence of tin the solubility of zinc in bismuth and that of bismuth in zinc are both materially increased by increment of temperature. A few experiments at a somewhat higher temperature, averaging near 800° , were also made, the results of which indicated a notable further increment in solubility; but, as considerable difficulty was experienced in getting even moderately concordant figures at this higher temperature, these observations were not carried far enough to enable average curves to be deduced. In every case the mixture of metals employed contained equal quantities of bismuth and zinc with varying proportions of tin; as in the case of the lead-zinc-tin alloys previously described, the effect of volatilisation and oxidation caused a small decrement in the proportion of zinc present in the compound ingots ultimately obtained relatively to the other two metals contained therein, especially in the experiments at the higher temperatures.

In analysing the alloys prepared, we found that the method of analysis used for lead-zinc-tin alloys could not be adopted without modification, because the stannic oxide formed by the action of nitric acid on the alloys retained variable amounts of bismuth in such a condition as not to be removed by copious washing with dilute nitric acid, or even by boiling therewith.* In some instances we weighed the crude stannic oxide retaining bismuth, and then fused it with sodium carbonate and sulphur, whereby sodium sulphostannate was formed, soluble in water, and bismuth sulphide insoluble therein. By precipitating the stannic sulphide by means of hydrochloric acid from the aqueous solution, and cautiously roasting it, the corrected weight of stannic oxide was obtained; this we found always concorded sensibly with the weight deduced by converting the undissolved bismuth sulphide into oxide (by dissolving in nitric acid, precipitating boiling with ammonium carbonate, and igniting the precipitate), and subtracting the weight of this from that of the crude stannic oxide. In other cases, we dissolved the alloy in hydrochloric acid containing a little nitric acid, diluted and treated with sulphuretted hydrogen, separating the tin

* It is noteworthy that no trace of lead appears to be thus retained by stannic oxide when alloys containing tin and lead are treated with nitric acid; on the other hand, when alloys containing antimony and lead are similarly treated, the undissolved antimony oxide retains a notable amount of lead, just as stannic oxide retains bismuth.

and bismuth thus precipitated with ammonium sulphide. Check experiments made by both methods yielded sensibly the same tin percentages. In order to determine the zinc after removal of bismuth and tin, we first precipitated it as sulphide, then dissolved in hydrochloric acid, and precipitated as carbonate, finally igniting and weighing as oxide; this method of treatment being adopted to prevent lime, &c., derived from the vessels used being weighed with the zinc, as would have been the case had the precipitation as sulphide been omitted. Any traces of oxide of iron contained in the zinc oxide were subsequently estimated and subtracted.

The following figures were derived from the examination of twenty-one compound ingots, the percentages being reckoned upon the sum of the weights of tin, bismuth, and zinc found as 100. With some of the lighter alloys, where zinc was the main constituent, only the tin and bismuth were determined, and the zinc taken by difference.

Series I. Temperature 600—700°.

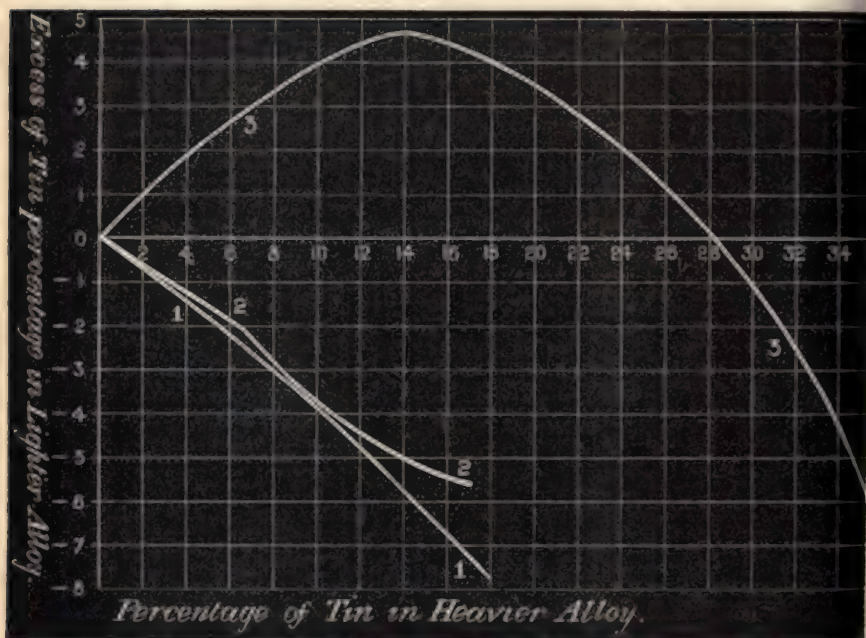
Percentage of tin in mixture before fusion.	Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in heavier.
	Tin.	Bismuth.	Zinc.	Tin.	Bismuth.	Zinc.	
0	0	85·72	14·28	0	1·32	97·68	0
2·5	3·23	80·27	16·50	1·98	3·45	94·57	-1·25
5·0	6·35	75·82	17·83	3·97	4·21	91·82	-2·38
8·3	10·38	70·52	19·10	6·35	5·53	88·12	-4·03
13·2	14·35	62·24	23·41	8·50	8·04	83·46	-5·85
15·9	18·01	56·10	25·89	10·24	10·90	78·86	-7·77
18·7	20·50	47·02	32·48				

Series II. Temperature 700—800°.

Percentage of tin in mixture before fusion.	Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in heavier.
	Tin.	Bismuth.	Zinc.	Tin.	Bismuth.	Zinc.	
0	0	84·82	15·18	0	2·47	97·53	0
5·3	6·29	76·20	17·51	4·24	5·24	90·52	-2·05
7·1	8·81	72·34	18·85	5·40	6·08	88·52	-3·41
8·8	10·35	67·71	21·94	6·29	7·35	86·36	-4·06
12·3	14·72	59·99	25·29	9·60	9·65	80·75	-5·12
15·3	16·87	54·03	29·10	11·57	12·62	75·81	-5·30

On plotting these figures as curves it is noticeable that, whilst the distribution of the tin between the heavier and lighter alloys formed is only slightly different according as the temperature is 650° or 750° , in each case the curve is of an entirely different character from that deducible from the previous experiments with lead, zinc, and tin. Instead of rising above the base line to a maximum and then falling again, ultimately crossing the base line and passing below it, each curve *lies completely below the base line*. Curves Nos. 1 and 2, fig. 1,

FIG. 1.

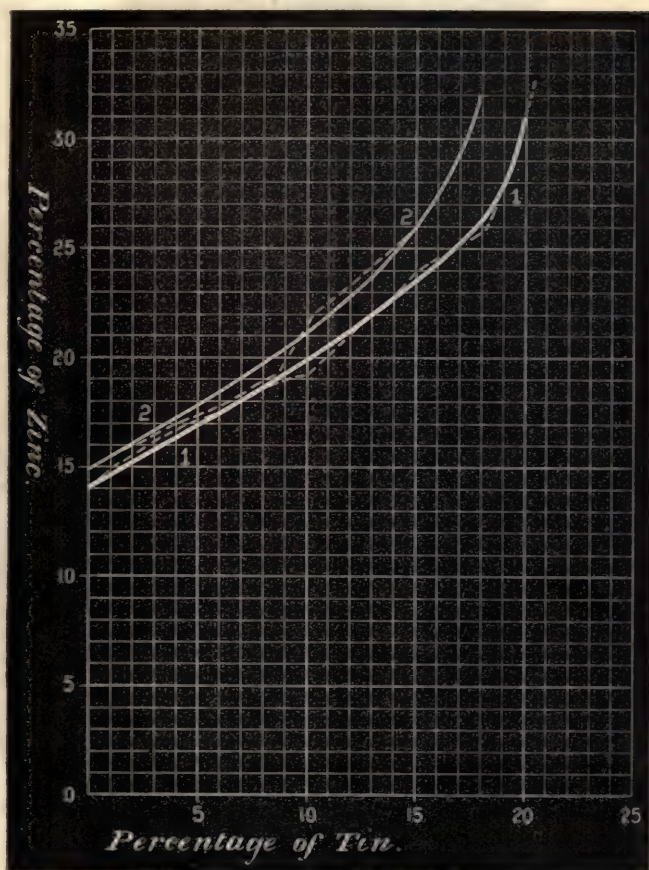


respectively represent the above values at 650° and 750° ; whilst No. 3 represents the corresponding curve obtained (Part I) with equal proportions of lead and zinc in the original mixtures at near 650° ; in each case the abscissæ are the percentages of tin in the heavier alloys, whilst the ordinates are the excesses of the tin percentages in the lighter alloys over the corresponding percentages in the heavier ones.

On the other hand, on plotting the curves representing the solubility of zinc in bismuth-tin (percentages of tin in heavier alloys as abscissæ, and those of zinc as ordinates), and of bismuth in zinc-tin (percentages of tin in lighter alloys as abscissæ, and those of bismuth as ordinates), it is obvious that the solubility increases in each case

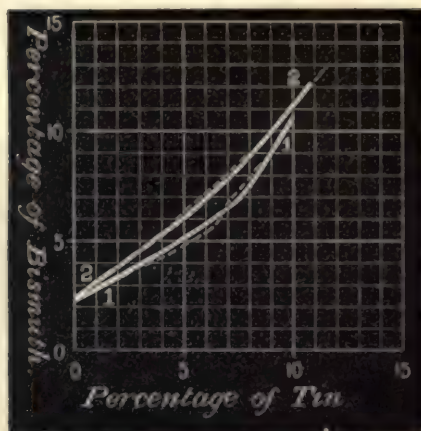
with the temperature, and that the amount of bismuth dissolved by zinc, or *vice versa*, regularly increases as the amount of tin present increases, just as in the case of the corresponding alloys containing lead instead of bismuth. Figs. 2 and 3 represent the solubility curves, the

FIG. 2.



curves marked 1 and 2 respectively indicating the values obtained at 650° and 750°; the dotted lines connect the actual points of observation, whilst the continuous lines represent the smoothed mean curves thence deduced. The following solubility tables are derived from these mean curves.

FIG. 3.



Solubility of Zinc in Bismuth-Tin.

At near 650°.			At near 750°.	
Percentage of tin.	Percentage of zinc.	Difference for 1 per cent.	Percentage of zinc.	Difference for 1 per cent.
0	14.28	—	15.18	—
2	15.20	0.46	16.10	0.46
4	16.20	0.50	17.10	0.50
6	17.30	0.55	18.20	0.55
8	18.50	0.60	19.50	0.65
10	19.80	0.65	21.00	0.75
12	21.20	0.70	22.80	0.90
14	22.70	0.75	24.90	1.05
16	24.30	0.80	27.50	1.30
17	25.15	0.85	29.40	1.90
18	26.20	1.05	32.00	2.60
19	28.10	1.90	—	—
20	31.00	2.90	—	—

Solubility of Bismuth in Zinc-Tin.

At near 650°.			At near 750°.	
Percentage of tin.	Percentage of bismuth.	Difference for 1 per cent.	Percentage of bismuth.	Difference for 1 per cent.
0	2·32	—	2·47	—
1	2·80	0·48	3·05	0·58
2	3·30	0·50	3·70	0·65
3	3·80	0·50	4·40	0·70
4	4·30	0·50	5·15	0·75
5	4·80	0·50	5·95	0·80
6	5·35	0·55	6·80	0·85
7	6·10	0·75	7·70	0·90
8	7·20	1·10	8·65	0·95
9	8·70	1·50	9·70	1·05
10	10·70	2·00	10·90	1·20
11	—	—	12·20	1·30

On comparing these mean curves with those described in Parts I and II, obtained with lead-zinc-tin alloys, it is evident that the solubility of zinc in bismuth is always greater than that in lead, whether tin be absent or present to a given extent in each case; and that, in the latter case, the rate of increment in the solubility of zinc in bismuth as the proportion of tin present increases is more rapid than the corresponding rate of increment in the solubility of zinc in lead. Precisely similar remarks apply to the solubility of bismuth in zinc.

Mixtures of Bismuth, Zinc, and Silver.

It has already been mentioned in Part II that mixtures of bismuth, zinc, and silver show the same remarkable behaviour as analogous mixtures of lead, zinc, and silver, leading to the conclusion that two definite compounds of zinc and silver are formed under appropriate conditions, indicated respectively by the formulæ AgZn_5 and Ag_4Zn_5 ; of which the first is characterised by being capable of dissolving more lead (or bismuth) than can either pure zinc or the second compound, and of being more soluble in lead (or bismuth) than either of these; also of being somewhat unstable, so that when a solution of lead (or bismuth) in AgZn_5 is kept molten for a long time it breaks up forming zinc and Ag_4Zn_5 , which being unable to dissolve all the lead originally present, causes the separation of lead from the liquid metal as a heavier alloy containing a little zinc and silver in solution. The second compound Ag_4Zn_5 is characterised by the peculiar red colour assumed by a recently cut or filed surface exposed to the air for awhile, and by

the circumstance that it is less soluble in lead (or bismuth), relatively to the zinc present, than either pure zinc or a mixture of zinc and silver in any other proportion.

Three series of experiments were made exactly corresponding with those previously described with lead-zinc-silver alloys in Part II; in each case the temperature was as near 750° as could be managed, ranging between 700° and 800°. The analysis of the alloys was made by dissolving in nitric acid, diluting, precipitating silver by hydrochloric acid, and washing the precipitate by decantation with hot dilute hydrochloric acid, in case any bismuth oxychloride might have separated. The filtrate was evaporated to a small bulk and treated with water, and the filtrate from the bismuth oxychloride formed treated with sulphuretted hydrogen; the bismuth in the sulphide and oxychloride thus obtained was determined by converting the joint precipitates into oxide by dissolving in nitric acid, precipitating boiling with ammonium carbonate, and igniting. The zinc was determined as in the former alloys, and corrected for small quantities of iron derived from the crucibles, &c.

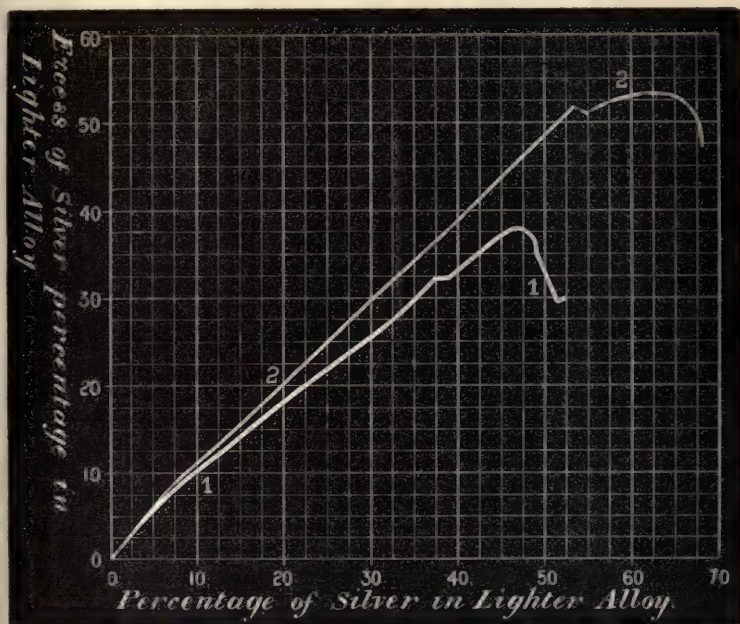
Series I.—Time of fusion, 8 hours. Temperature 700—800°.

Percentage of silver in mixture before fusion.	Heavier alloy.			Lighter alloy.			Excess of silver percentage in lighter alloy over that in heavier.
	Silver.	Bismuth.	Zinc.	Silver.	Bismuth.	Zinc.	
0	0	84·82	15·18	0	2·47	97·53	0
2	0·04	85·05	14·91	3·83	4·17	92·00	3·79
4	0·20	84·39	15·41	10·44	5·71	83·85	10·24
6·25	0·96	79·09	19·65	14·38	7·16	78·46	13·42
8·75	2·29	74·09	23·62	17·19	10·60	72·21	14·90
12·5	3·32	76·48	20·20	22·36	13·39	64·25	19·04
14	3·49	77·95	18·56	26·42	11·04	62·54	22·93
15·5	4·56	74·92	20·52	30·30	6·40	63·30	25·74
18	4·95	76·10	18·95	34·18	5·63	60·19	29·23
21	5·08	79·28	15·64	37·06	5·34	57·60	31·98
25	6·84	77·24	15·92	38·80	5·93	55·27	31·96
31	8·98	77·99	13·03	46·31	8·67	45·02	37·33
33	12·71	76·19	11·10	47·94	10·05	42·01	35·23
36	14·39	75·24	10·37	48·50	10·50	41·00	34·11
38	22·96	62·72	14·32	51·34	14·34	34·32	28·38
39·5	23·10	61·30	15·60	51·78	15·41	32·81	28·68
40·5	29·27	53·08	17·65	—	—	—	—
45	39·90	41·03	19·07	—	—	—	—

The above figures were derived from a series of twenty-eight compound ingots, each prepared from equal weights of zinc and bismuth with varying proportions of silver. They show almost exactly the

same peculiarities as the corresponding series obtained with lead, zinc, and silver described in Part II; thus the silver-distribution curve is of sensibly the same character, the excess of silver in the lighter alloy over that in the heavier being at first extremely great, but later on lessening, until a maximum elevation of the curve above the base line is attained, after which the curve again descends. Curve No. 1, fig. 4, indicates this, No. 2 being the corresponding curve from the

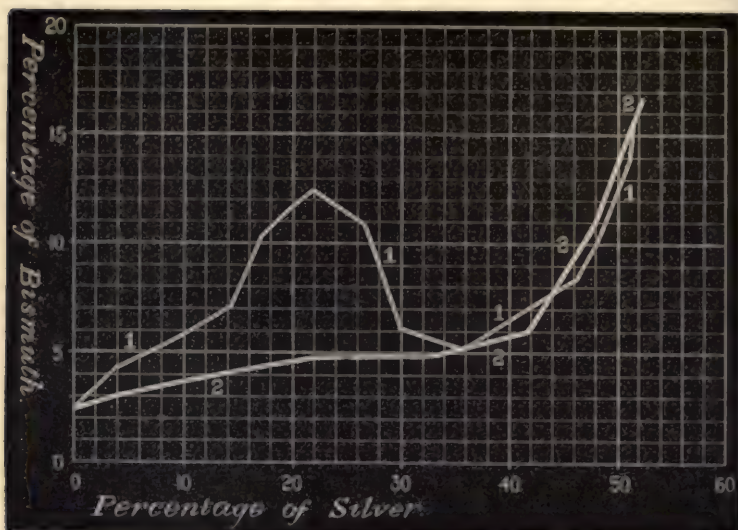
FIG. 4.



lead-zinc-silver alloys (Part II), the abscissæ in each being the percentages of silver in the *lighter* alloys, and the ordinates the excesses of silver percentage in the lighter alloys over those in the heavier ones. Similarly, the curve traced out by plotting the silver and bismuth percentages in the lighter alloys as abscissæ and ordinates respectively (No. 1, fig. 5) exhibits the same feature of rapid rise to a maximum, subsequent fall to a minimum but little above the starting level, and later continuous rise. The position of the first maximum, moreover, is close to that indicating the relationship AgZn_5 , just as with the former alloys.

Silver.	Zinc.	Ratio of zinc to silver.
22.36	64.25	1 to 0.348
Calculated for AgZn_5		
		1 to 0.332

FIG. 5.



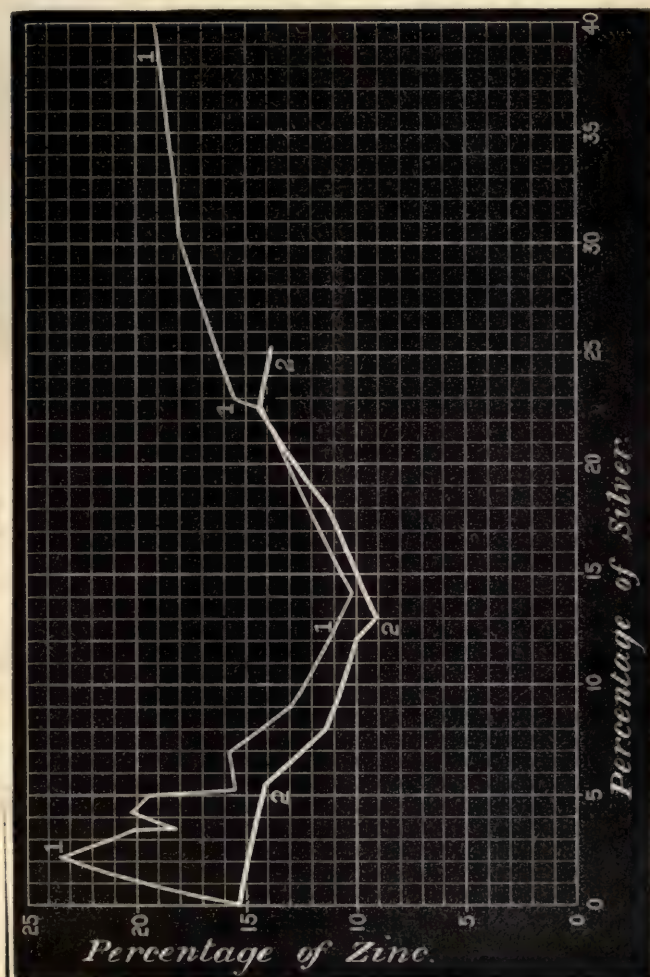
Further, those alloys where the silver and zinc present were approximately in the proportions denoted by Ag_4Zn_5 showed the same marked reddish hue on exposure to the air for a short time, after filing bright, as was observed with the analogous lead-zinc-silver alloys; thus the two alloys containing the following percentages showed it strongly—

Silver.	Bismuth.	Zinc.	Ratio of zinc to silver.
47.94	10.05	42.01	1.14
51.34	14.34	34.32	1.49
Mean.....			1.315
Calculated for Ag_4Zn_5			1.33

as also did some others, the compositions of which lay between these limits, which correspond respectively with 84 per cent. of Ag_4Zn_5 with a little excess of zinc and some bismuth, and with 80 per cent. of Ag_4Zn_5 with excess of silver and some bismuth. On the other hand, alloys containing somewhat larger excesses of silver or zinc showed only a much paler tint, whilst little or no coloration was visible with alloys where the percentage of Ag_4Zn_5 fell below about 65 per cent. We prepared some binary alloys of silver and zinc consisting mainly of Ag_4Zn_5 , with but small excess of either silver or zinc; these showed the coloration strongly.

Again, the curve obtained by plotting the silver and zinc percentages in the heavier alloys as abscissæ and ordinates respectively exhibits (No. 1, fig. 6), first, a rapid rise in the quantity of zinc pre-

FIG. 6.



sent, followed by a fall to such an extent that the zinc present, reckoned per unit of bismuth, diminishes down again to an amount *considerably below that present in the binary bismuth-zinc alloy* containing no silver; a minimum of zinc is finally attained, after which the zinc present relatively to the bismuth rises continuously.

Silver.	Bismuth.	Zinc.	Zinc per unit of bismuth.
0	84·82	15·18	0·179
2·29	74·09	23·62 (maximum)	0·319
4·95	76·10	18·95	0·249
6·84	77·24	15·92	0·206
8·98	77·99	13·03	0·167
12·71	76·19	11·10	0·145
14·39	75·24	10·37 (minimum)	0·138
22·96	63·72	14·32	0·225
29·77	53·08	17·65	0·332
39·90	41·03	19·07	0·465

This reduction of zinc to a minimum is far more strongly marked than in the case of the lead-zinc-silver alloys; the point where it occurs is not far from that where the silver and zinc present are in the ratio indicated by the formula Ag_4Zn_5 .

Silver.	Zinc.	Ratio of zinc to silver.
14·39	10·37	1 to 1·39
Calculated for Ag_4Zn_5 1 to 1·33		

Whence it may be concluded that, whereas AgZn_5 is much more soluble in fused bismuth than pure zinc (relatively to the zinc present in each case), the same is not the case with the compound Ag_4Zn_5 , which dissolves in bismuth to a much less extent than would zinc alone in the absence of silver.

Series II was prepared in precisely the same way as the corresponding series with the lead-silver-zinc alloys; i.e., mixtures of the three metals were made and fused for eight hours as in Series I; the compound ingots thus obtained were cut in two so as to separate from one another the lighter and heavier alloys thus formed, and the former parts separately fused at the same temperature (700—800°) for

Series II.—Limiting Composition of Lighter Alloys.

Silver.	Bismuth.	Zinc.
3·93	2·95	93·12
13·72	4·15	82·13
21·24	4·83	73·93
33·27	4·89	61·84
37·50	5·34	57·16
41·43	5·90	52·67
48·02	11·12	40·86
52·01	16·47	31·52

another eight hours. In this way a further separation was brought about in the case of the alloys prepared with smaller proportions of silver, but no material alteration in the case of those made with larger proportions, just as with the lead-silver-zinc alloys. On plotting the results, a curve was obtained (No. 2, fig. 5) from which the first maximum, at approximately the point representing the compound AgZn_5 , had completely disappeared, as had also the subsequent fall, the curve exhibiting a progressive rise from beginning to end.

Series III was similarly made with the heavier alloys thus separated from the lighter ones; the results, when plotted (curve No. 2, fig. 6), showed that the abnormally large percentages of zinc observed in the earlier part of the series had disappeared, whilst the diminution in amount of zinc dissolved relatively to the bismuth present down to a minimum and subsequent rise again was still well marked, the position of the minimum corresponding, as before, with a ratio of zinc to silver not far from that indicated by the formula Ag_4Zn_5 .

Series III.—Limiting Composition of Heavier Alloys.

Silver.	Bismuth.	Zinc.	Zinc per unit of bismuth.
0	84·82	15·18	0·179
5·42	80·62	13·96	0·173
7·65	80·70	11·65	0·144
11·71	78·00	10·29	0·132
13·14	77·85	9·01	0·116 (minimum)
17·98	71·00	11·02	0·155
22·96	62·70	14·34	0·228
25·11	61·39	13·50	0·220

Position of minimum:—

Silver.	Zinc.	Ratio of zinc to silver.
13·14	9·01	1 to 1·45
Calculated for Ag_4Zn_5		1 to 1·33
,, ,, AgZn		1 to 1·66

The following tables represent the mean solubility curves deduced from all the preceding results, omitting the earlier alloys in Series I, where, owing to the presence of undecomposed AgZn_5 , excess of lead was present in the lighter alloys, and excess of zinc in the heavier ones.

Solubility of zinc in bismuth-silver.			Solubility of bismuth in zinc-silver.		
Percentage of silver.	Percentage of zinc.	Difference for 1 per cent.	Percentage of silver.	Percentage of bismuth.	Difference for 1 per cent.
0	15.18	—	0	2.47	—
1	14.70	-0.48	5	2.82	0.07
2	14.20	-0.5	10	3.17	0.07
5	12.70	-0.5	15	3.52	0.07
6	12.25	-0.45	20	3.87	0.07
11	10.00	-0.45	25	4.27	0.08
12	9.65	-0.35	30	4.67	0.08
13	9.50	-0.15	35	5.07	0.08
14	9.50	0	36	5.15	0.08
15	9.65	+0.15	37	5.23	0.08
16	10.00	+0.35	38	5.40	0.17
17	10.50	+0.5	39	5.70	0.30
18	11.20	+0.7	40	6.10	0.40
19	12.00	+0.8	41	6.50	0.40
21	13.60	+0.8	42	7.00	0.50
22	14.30	+0.7	43	7.50	0.50
23	14.95	+0.65	44	8.00	0.50
24	15.50	+0.55	45	8.60	0.60
25	16.00	+0.5	46	9.20	0.60
26	16.40	+0.4	47	9.90	0.70
27	16.80	+0.4	48	10.60	0.70
28	17.10	+0.3	49	11.40	0.80
30	17.70	+0.3	50	12.30	0.90
31	17.90	+0.2	51	13.70	1.40
32	18.10	+0.2	52	16.30	2.60
33	18.25	+0.15			
36	18.70	+0.15			
37	18.80	+0.1			
40	19.10	+0.1			

On comparing together the relative effects on the solubility of bismuth in zinc and of zinc in bismuth produced by the simultaneous presence of tin or of silver the same general result is deduced as in the case of lead-silver-zinc and lead-tin-zinc alloys, viz., that in each instance the solubility is considerably more increased by the presence of a given proportion of tin than by that of the same amount of silver. If 100 parts of zinc can take up m parts of bismuth in presence of x parts of tin (or silver), and if 100 parts of bismuth can take up n parts of zinc in presence of x parts of tin (or silver), then the following tables give the correlated values of m , n , and x , these values being minima in the case of alloys containing silver, i.e., being deduced from those experiments where the influence of the presence of the compound AgZn_5 in increasing solubility was eliminated.

x .	Zinc dissolved by 100 parts of bismuth in presence of x parts of tin (or silver).			Bismuth dissolved by 100 parts of zinc in presence of x parts of tin (or silver).		
	Tin at 650°.	Tin at 750°.	Silver at 750°.	Tin at 650°.	Tin at 750°.	Silver at 750°.
	<i>n.</i> Diff.	<i>n.</i> Diff.	<i>n.</i> Diff.	<i>m.</i> Diff.	<i>m.</i> Diff.	<i>m.</i> Diff.
0	16·8 —	17·9 —	17·9 —	2·37 —	2·53 —	2·53 —
2·5	18·5 1·7	19·6 1·7	16·8 -1·1	3·70 1·33	4·30 1·77	2·75 0·22
5	20·3 1·8	21·4 1·8	15·8 -1·0	5·20 1·50	6·20 1·90	3·00 0·25
7·5	22·3 2·0	23·5 2·1	14·9 -0·9	7·00 1·80	8·40 2·20	3·25 0·25
10	24·4 2·1	26·0 2·5	14·0 -0·9	9·50 2·50	10·80 2·40	3·50 0·25
12·5	26·6 2·2	28·5 2·5	13·2 -0·8	13·40 3·90	13·70 2·90	3·75 0·25
15	28·9 2·3	31·1 2·6	12·6 -0·4		17·00 3·30	4·00 0·25
17·5	31·3 2·4	34·0 2·9	12·3 -0·3			4·25 0·25
20	33·7 2·4	37·2 3·2	12·8 +0·5			4·50 0·25
22·5	36·3 2·6	41·2 4·0	13·8 +1·0			4·75 0·25
25	39·1 2·8	46·2 5·0	15·3 +1·5			5·00 0·25
27·5	42·0 2·9	52·3 6·1	17·2 +1·9			5·25 0·25
30	45·0 3·0		19·3 +2·1			5·50 0·25
35	52·1 7·1		22·8 +3·5			6·00 0·50
40	61·8 9·7		25·8 +3·0			6·50 0·50
50			30·7 +4·9			7·60 1·10
60			34·7 +4·0			8·90 1·30
70			38·2 +3·5			10·50 1·60
80			41·4 +3·2			13·00 2·50
90			44·5 +3·1			16·10 3·10
100			47·5 +3·0			19·60 3·50

The curves indicated by the continuous lines in figs. 7 and 8 represent these numbers, the dotted lines representing the analogous curves described in Part II, obtained with lead-zinc-tin and lead-zinc-silver alloys. In fig. 7, curve No. 1 indicates the amounts of zinc dissolved at 650° by 100 parts of bismuth in presence of x parts of tin, whilst No. 2 represents the corresponding amounts dissolved at the same temperature by lead. Nos. 3 and 4 similarly represent the amounts of zinc dissolved at 750° by bismuth and at 800° by lead respectively. Nos. 5 and 6 indicate the amounts of zinc dissolved in presence of x parts of silver by 100 parts of bismuth at 750° and of lead at 800° respectively.

In fig. 8, curves Nos. 1 and 2 respectively represent the amounts of bismuth and lead dissolved by 100 parts of zinc in presence of x parts of tin at 650°. Nos. 3 and 4 represent similarly the bismuth dissolved at 750° and the lead at 800° respectively. Nos. 5 and 6 indicate the bismuth dissolved at 750° and the lead at 800° in presence of x parts of silver.

FIG. 7.

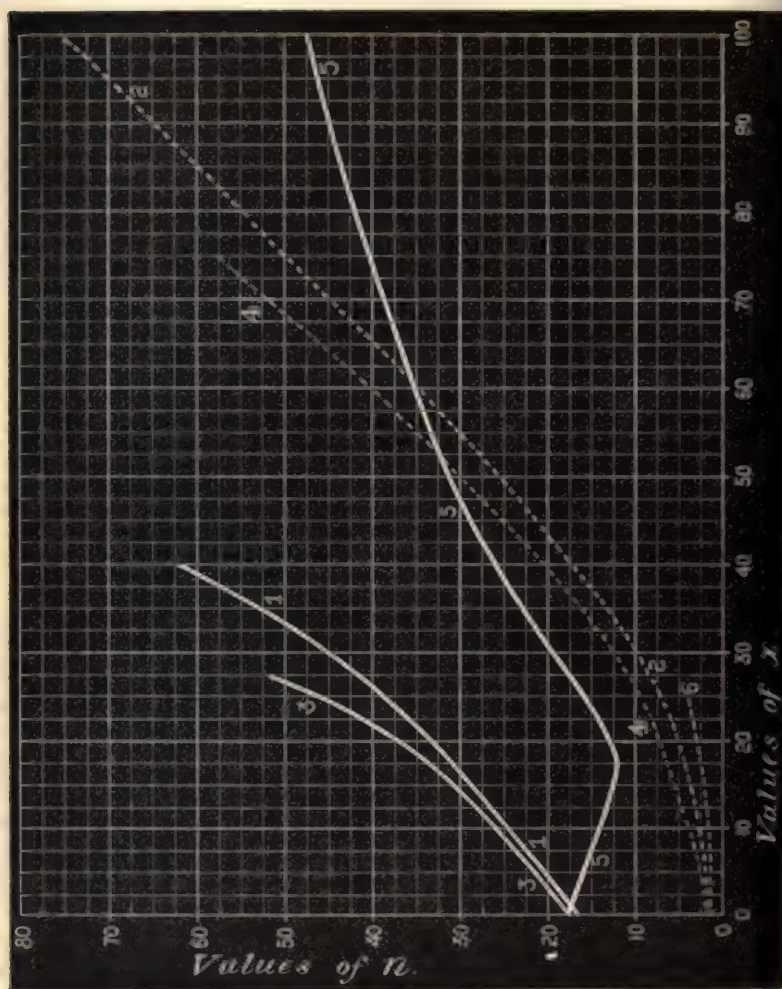
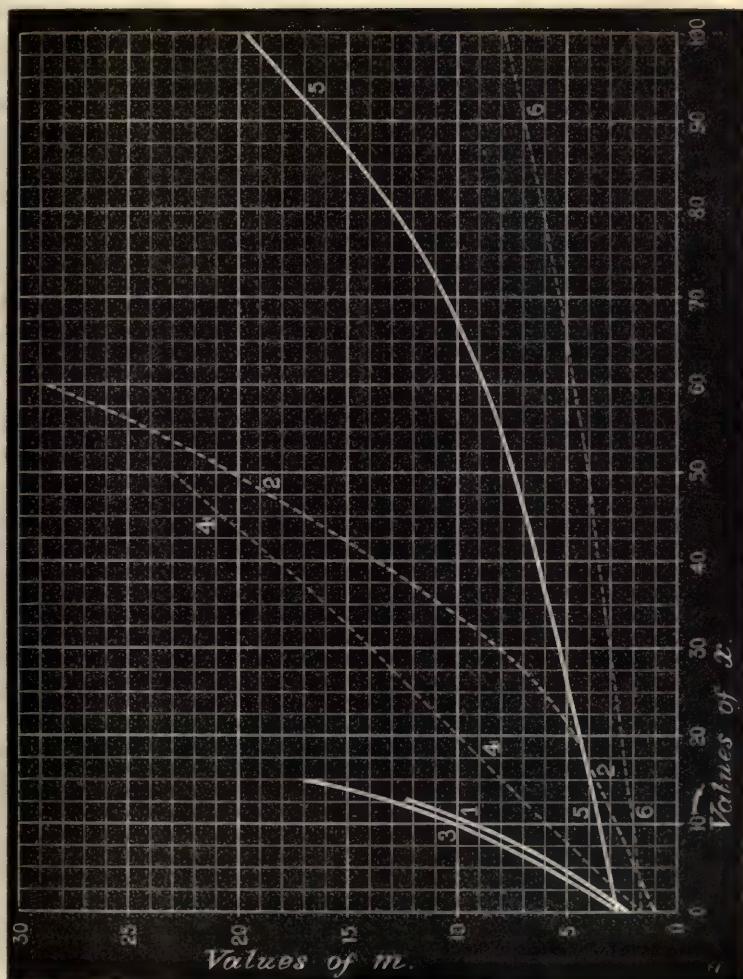


FIG. 8.



III. "On Certain Ternary Alloys. Part IV. On a Method of Graphical Representation (suggested by Sir G. G. Stokes) of the way in which certain Fused Mixtures of Three Metals divide themselves into Two different Ternary Alloys; with further Experiments suggested thereby." By C. R. ALDER WRIGHT, D.Sc., F.R.S., Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School; C. THOMPSON, F.I.C., F.C.S.; and J. T. LEON, B.Sc., F.C.S., Assistant Lecturer on Physics and Demonstrator of Chemistry in St. Mary's Hospital Medical School. Received January 29, 1891.

A method of graphically representing the results of the experiments described in the previous portions of these researches has been kindly suggested to one of us by Sir G. G. Stokes, founded on a principle which he regards as self-evident. We subjoin a note which he has been so good as to draw up for us, explaining the application of this method, and then describe some further experiments which we have instituted with a view to test the correctness of the assumed principle.

Note on a Graphical Representation of the Results of Dr. Alder Wright's Experiments on Ternary Alloys. By Sir G. G. STOKES, Bart., F.R.S.

Suppose three liquids such as water, ether, and alcohol, of which the third is miscible in all proportions with either of the others, are mixed together, the temperature being kept constant. According to circumstances, the mixture forms a single liquid mass, or separates into two. In the latter case, if we suppose that the liquids had been merely gently poured together, and imagine the upper and under portions separately to be homogeneous to start with, this state of things would not remain; an alteration of composition would take place close to the surface of separation on both sides, depending on the relative solubilities, &c., of the ingredients. If now the two altered strata were mixed up with the rest of the portions to which they respectively belong, the same thing would go on again, and so on till a condition was reached in which what we may call an equilibrium of composition on the two sides of the surface of separation had been attained. As this equilibrium depends only on the molecular forces, which are insensible at sensible distances, it is evident that the equilibrium would not be disturbed by removing a part of either the upper or the under liquid, or by adding to it liquid of exactly the

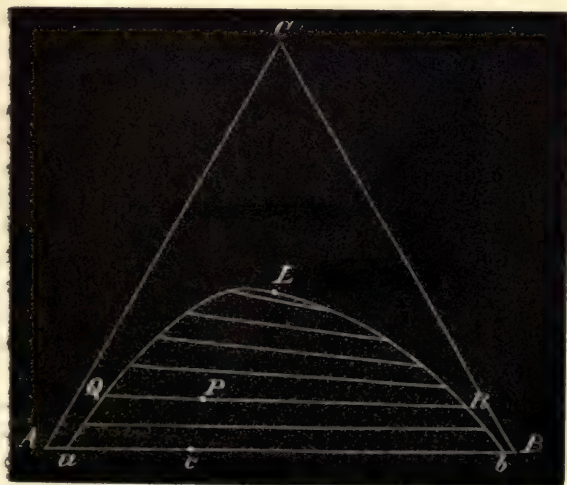
same composition as itself. This final state would take place only very slowly in the manner conceived above; but if the mixture be well agitated the total surface of separation, where alone the change of composition can go on, is greatly increased, and, moreover, the altered strata are mixed up with the rest of the liquids to which they respectively belong, so that the final state is reached comparatively quickly. I think I have seen an experimental verification of this anticipation, namely, that equilibrium depends only on the compositions of the upper and lower mixtures, and not on their quantities, in a French serial, but I have not the reference.

The same principles would apply to ternary alloys, which form a homogeneous mass, or separate into two, as the case may be; but of course the difficulty of preserving a constant temperature is much greater, as well as that of giving sufficient agitation to bring about the final condition.

It seemed to me that, for giving an insight into the results of experiments with ternary alloys, a mode of graphical representation might be usefully employed which is already well known. It is the same as that which Maxwell used for the composition of colours, at least with one slight addition. In this way the whole of the circumstances of the experiment, so far as they are material, would be exhibited to the eye.

Let A, B, C be three liquids, such as water, ether, alcohol, or else lead, zinc, tin, in fusion, of which the third (which for distinction may be called the solvent) may be mixed in all proportions with either the first or the second. Take a triangle, ABC (fig. 1), which may be of

FIG. 1.



any form, but is most conveniently chosen equilateral; and, to represent the composition of any mixture of the three, imagine weights equal to those of the substances A, B, C placed at the points A, B, C, and find their centre of gravity, P. To each different set of proportions $A : B : C$ (the letters here denoting weights) will correspond a different position of P, which point will serve to represent to the eye the composition of an actual or ideal alloy (supposing the substances to be metals) formed of the three metals in the given proportions. If the quantity of the solvent be sufficient, P will represent on the diagram the composition of an actual alloy. If it be insufficient, the alloy represented as to composition by P will be ideal only; and on attempting to form it the mass will separate into two layers. If we suppose the agitation to have been sufficient, there will be equilibrium of solution at the surface of junction, and the mass will have reached its final state. Supposing this condition to have been attained, let the two portions be analysed, and the points Q, R representing their compositions be laid down on the diagram, and joined by a straight line. From the construction, this line must pass through the point P if there has been no loss by volatilisation or oxidation. Let the same thing be done for several other proportions of the ingredients. Then the points Q, R will lie in a curve $aQLRb$, cutting AB in two points a, b , which represent, the first, a saturated solution of B in A, the second, a saturated solution of A in B. Call this curve the *critical curve*, and the lines such as QR *tie-lines*, or simply *ties*. Then the critical curve and the system of ties will represent the complete result of the experiments, supposing them to have been exactly made. Alloys of a pair may conveniently be called *conjugate*. Intermediate tie-lines may be interpolated by eye; or if we prefer we may substitute for the system of ties their envelope, on which plan the result of the experiments would be completely represented by two curves, the critical curve and the envelope.

The critical curve separates mixtures of which alloys can actually be formed from those on attempting to form an alloy of which the mass separates into two layers. In the latter case, if through P we draw a tangent to the envelope, cutting the critical curve in Q, R, the points Q, R will represent the compositions of the portions into which the mass separates, while their weights will be as PR to PQ.

If L be the limiting position of the chord QR, or, in other words, the point of contact with the critical curve of a common tangent to it and the envelope, as P tends to coincide with L, the two strata into which the mass separates tend to become identical in nature. If we take a mixture of A and B, represented by a point c in ab , and continually increase the quantity of C from 0, the point P will ascend from c towards C until it reaches the critical curve. At this stage the quantity of the second alloy has just dwindled away to nothing,

its nature, so long as there was any of it left, differing from that of the other alloy. If, however, the point *c* lies in the line CL, on increasing the quantity of C the two alloys merge into one.

On communicating to Dr. Alder Wright this mode of graphical representation, he tried it on a large scale on the results of two pairs of series from the former experiments. In one pair the temperature was 650° , and the proportion by weight of zinc to lead was 2 to 1 in the first case, and 1 to 2 in the second. In the other pair the weights of zinc and lead were equal, and the temperature 650° in one case and 800° in the other. In the first pair the agreement of the critical curves was very good, but the agreement in the direction of the ties was not by any means equally good. In the upper part of the figure, corresponding to the case in which there was a considerable quantity of tin, though not enough by any means to prevent the formation of two layers in the entire mass, the difference of inclination ranged to about 5° , the ties in the first case being inclined to those in the second as if they had been turned round in the direction of a line passing through the lead corner of the triangle, and turning round in the direction from lead-zinc to lead-tin. In the second pair of series in which the weights of lead and zinc were equal, and the temperature was 650° in the first case and 800° in the second, the critical curve for 800° was of the same general character as that for 650° , but lay a little inside it, which is just what was to be expected, on account of the increase of solubility attending the higher temperature. Moreover, the critical curve for 650° agreed very fairly with those for the same temperature in the first pair, notwithstanding the difference in the proportion of lead to zinc in the three cases.

I had not anticipated the greater accordance existing between the critical curves in different cases for the same temperature than that shown in the direction of the ties. But, when the plottings revealed it, it seemed to me that the cause was not far to seek. When the molten mass has as yet been but slightly stirred, the superposed alloys, supposed to be severally homogeneous, will most likely be represented on the diagram by points, one or both of which lie outside the critical curve. In this condition an alloy represented by an external point, having the metal C to spare, will be capable of dissolving bodily a portion of the other. This process accordingly, being something analogous to the solution of a salt till saturation is obtained, will go on as the stirring proceeds, and be sensibly complete in a moderate time. The two alloys will then be represented by two points lying on the critical curve. Such alloys may be said to be *associated*. But the passage from merely associated to truly conjugate alloys, as the stirring proceeds, is likely to be decidedly slower. For now neither alloy can bodily dissolve any portion, however small, of

the other; there can only be an interchange of constituents across the surface of separation.

The critical curve may be otherwise defined as the curve expressing the saturation of the solvent C with a mixture in given variable proportion of the remaining substances A, B. That it is really such, a little consideration suffices to show. The determination accordingly of the critical curve furnishes us with definite information, even though we do not go into the ulterior question of the condition of conjugation.

Perhaps the attainment of true conjugation might involve more stirring than would be practically feasible with molten metallic mixtures. The most hopeful way would seem to be to fuse the mass at a higher temperature than that intended for the experiment, stirring it well, and then let down the temperature to that intended, stirring all the time, and avoiding too rapid a fall of the temperature.

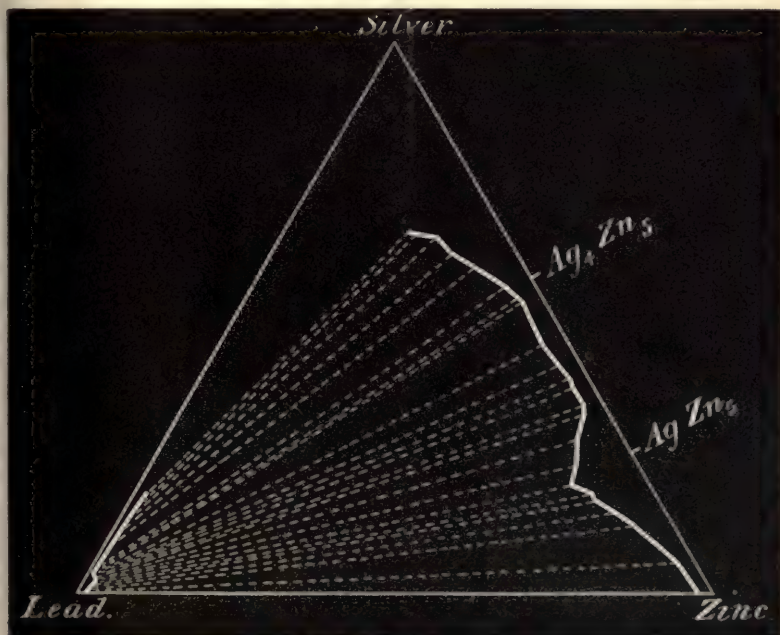
If truly conjugate alloys were obtained, and portions of each were taken and fused together at the temperature at which the alloys were made, the compositions ought to be the same as before. But if the alloys were merely associated, then, even if the stirring in the second part of the experiment were sufficient to ensure conjugation, the compositions would not be the same as the original, nor would they be independent of the proportion of the two alloys which the operator took for fusing together.

The triangular method of representation described by Sir G. G. Stokes in the above note obviously possesses several advantages, inasmuch as it represents in one diagram simultaneously a number of results which the ordinary curves drawn with abscissæ and ordinates can only partially indicate, consequently necessitating several different curves being drawn in order to represent graphically the entire set of results; thus the two branches of the "critical curve," obtained by directly plotting the figures yielded on analysis of the lowest and uppermost portions respectively of the compound ingot formed (in the case of a mixture separating into two different ternary alloys), represent the two solubility curves (*e.g.*, of zinc in lead-tin and of lead in zinc-tin), whilst the "ties" or "tie-lines" indicate, according as they slope to one side or the other, the relative proportions of the "solvent" (*e.g.*, tin) in the heavier and lighter alloys; so that, when (as in the case of mixtures of lead, zinc, and tin) with certain proportions of "solvent" the heavier alloy, and with other proportions the lighter one, contains the larger percentage, this variation is at once indicated to the eye by the change in direction of slope of the tie-lines (compare fig. 3). Further, when once the critical curve for

given temperature has been laid down, it is at once evident by inspection whether a given mixture of metals will furnish a "real" alloy (not separating into two different ternary mixtures), or only an "ideal" alloy (*i.e.*, one not capable of existence, and consequently separating into two different ternary alloys); for, in the one case, the centre of gravity of the weights of the three metals respectively placed at the angles of the triangle will fall outside, and, in the other case, inside, the space enclosed between the critical curve and the base of the triangle.

Again, any abnormal results due to the formation of definite chemical compounds (such as the silver-zinc compounds AgZn_5 and Ag_4Zn_5 , shown to exist by the experiments described in Parts II and III) are equally indicated by the irregularity of the outline of the critical curve deduced: thus fig. 2 indicates on Sir G. G. Stokes's

FIG. 2.



system some of the results obtained with zinc-lead-silver alloys (Part II, 'Roy. Soc. Proc.,' vol. 48, p. 33, Series I); the branch of the critical curve corresponding with the lighter alloys obviously indicates the first maximum of dissolved lead (at a point near to that corresponding with AgZn), the subsequent fall, and the point where marked increment again becomes apparent (near that corresponding

with Ag_4Zn_5) in the same way as the abscissa and ordinate curve shown in fig. 5, Part II, p. 35. It is noteworthy, however, that whilst the direction of the slope of the ties indicates that throughout the lighter alloy contains more silver than the heavier one, the triangular graphical representation does not clearly indicate that the difference in silver percentage between the lighter and heavier alloys rises to a maximum and then diminishes again, as is distinctly shown by the ordinary method with abscissæ and ordinates, as depicted in fig. 4, Part II, p. 35.* Precisely the same remarks apply if the analogous results obtained with bismuth-zinc-silver alloys described in Part III are similarly plotted.

In addition, however, to the employment of this improved method of graphical representation, Sir G. G. Stokes deduces from *a priori* considerations an important general principle, viz., that when a sufficient amount of intermixture of the constituent metals has taken place a state of equilibrium is arrived at (the temperature being constant throughout), such that the presence of one ternary alloy in no way affects the composition of the other; so that the addition or subtraction of a further quantity of either alloy, or of any mixture of the two, does not affect the compositions, but only the relative quantities present, of the two alloys; whence, if any given weights of the two fused alloys be intermixed, the same weights of the same alloys will separate again from one another by gravitation on standing. If, therefore, two given alloys, A and B, be thus related (truly *conjugate*), and in any particular experiment carried out until equilibrium is reached one of these alloys, A, be formed, the other alloy, B, must necessarily be also produced; and this must be the case no matter what may have been the relative proportions subsisting between the three metals in the mixture originally employed.

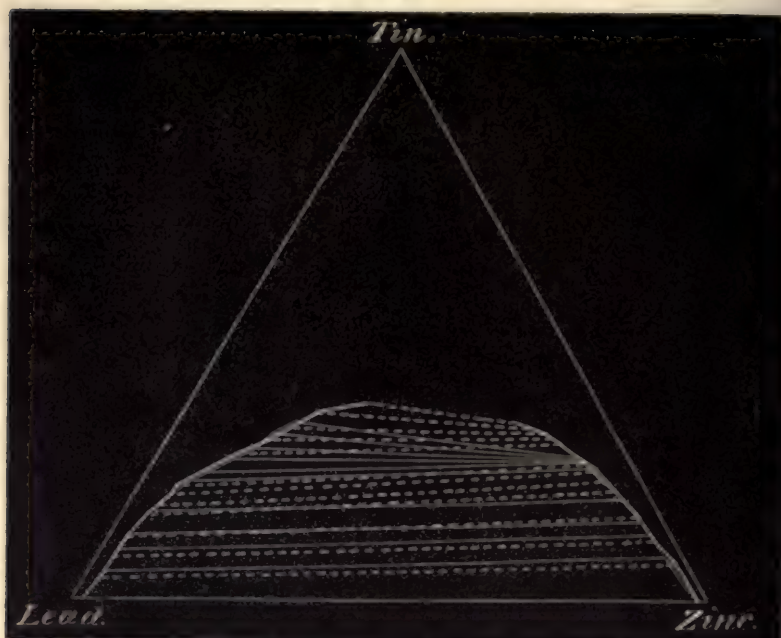
It appeared to us of considerable interest to examine from the experimental point of view whether this general principle can be

* [Sir G. G. Stokes has pointed out to me that the diagram, fig. 1, shows at once that, inasmuch as the difference between the percentages of the solvent in two conjugate alloys vanishes for the pair, a, b , being *nil* for each, and again for the pair which merge into one, represented by the point L, it must necessarily be a maximum for some intermediate pair; and also that, in order to preserve the continuity of conditions, we must, in crossing L, pass from the upper alloy to the lower, and *vice versâ*. Hence, if the entire system of ties could be determined, so as to obtain every possible pair of conjugate points lying, one on one side, the other on the other side, of L, and if these values were plotted on the abscissa and ordinate system, the curve representing the difference between the percentages of the solvent, after having ascended and attained a maximum elevation, must descend again to the base line at a point corresponding with L. If we wish to continue the curve beyond that point, we must now take the ordinates negative instead of positive, the same in magnitude as before, and the curve having crossed the base line, and attained a minimum elevation, will ultimately ascend again to the final point on the base line.—C. R. A. W., February 25, 1891.]

verified in practice, or whether interfering causes prevent anything more than demonstrations of approximate correctness being obtained; the more so that some of the results previously obtained by two of us do not appear to be in harmony with Sir G. G. Stokes's proposition. In Part I ('Roy. Soc. Proc.,' vol. 45, p. 461) three series of experiments were described, made with lead, tin, and zinc, where the ratio of lead to zinc was 2 to 1, 1 to 1, and 1 to 2, in the three series respectively; and the figures obtained led us to the conclusion that "whilst an indefinite number of different mixtures may be prepared, each one of which will give the same heavier alloy, the lighter alloy simultaneously formed will be different in each case; and conversely:" a deduction obviously incompatible with Sir G. G. Stokes's proposition. On the other hand, it is argued by Sir G. G. Stokes that these experiments do not necessarily prove anything more than the extreme difficulty experienced whilst making experiments with fused metals in obtaining such an intimate intermixture as to bring about the condition of perfect equilibrium between the two alloys formed in any given instance; and that, in point of fact, the differences observed in the compositions of the various lighter alloys associated with a given heavier one, or *vice versâ*, are not greater than might reasonably be expected were equilibrium not perfectly attained in some or all of the observations. Further, the fact that the differences are *always in the same general direction* tends to indicate that some constant interfering cause is at work; thus, when curves were plotted (Part I, fig. 5, p. 476) with the tin percentages in the heavier alloys as abscissæ, and the excesses of tin percentage in the lighter alloys over those in the heavier ones as ordinates, the curve deduced from the series of experiments where the ratio of lead to zinc in the original mixture of metals was 2 to 1 *underlay* that similarly obtained in the second series, where the ratio was 1 to 1, which again *underlay* that deduced from the third series, where the ratio was 1 to 2; whereas all three curves should have coincided were Sir G. G. Stokes's proposition correct, and all interfering causes completely eliminated.

An analogous result is obtained when the analytical figures are plotted on Sir G. G. Stokes's triangular system. Fig. 3 represents the plottings thus obtained of the two series where the ratio of zinc to lead was 2 to 1 and 1 to 2 respectively (Part I, 'Roy. Soc. Proc.,' vol. 45, Series IV, p. 474, and Series VI, p. 475) the temperature throughout being near to 650°. The ties in the first case are indicated by dotted lines, and in the second by continuous ones. Obviously the critical curves deduced from the two sets of observations respectively do not differ very markedly; but the angles of slope of the ties are not identical, so that a given heavier alloy is not conjoined with the same lighter one (nor *vice versâ*) in the two cases; whilst the direction of the variation is the same throughout.

FIG. 3.



In order, if possible, to obtain experimental evidence of the truth or otherwise of the general proposition arrived at by Sir G. G. Stokes, as well as some explanation of the deviation therefrom of these previous results, we first of all carried out various further experiments with mixtures of lead, zinc, and tin, employing additional precautions to minimise errors due to imperfect intermixture, more especially by continuing for much longer periods of time the process of agitation of the fused metals by vigorous stirring; the results, however, did not differ materially from the previous ones, and indicated generally that the composition of the heavier alloy practically obtained associated with a given lighter one, or *vice versa*, was subject to fluctuation within certain not very wide limits, according to the proportion subsisting between lead and zinc in the original mixture employed; but whether this result was brought about by interfering causes, or was possibly due to the not absolute correctness of Sir G. G. Stokes's principle, the experiments did not enable us to decide. In the hope of eliminating disturbing causes, we next endeavoured to carry out analogous observations at the ordinary temperature with liquids not metallic in their nature, but resembling the metals tin, lead, and zinc from the point of view of

their relative solubilities, *i.e.*, two of the liquids being only miscible together to limited extents (like lead and zinc), whilst the third was miscible in all proportions with either of the others separately. The difficulty of making sufficiently accurate analyses of the ternary mixtures thus obtained prevented our using several such groups of liquids, which at first sight suggested themselves, more particularly mixtures of *alcohol, water, and ether*; but we found that *chloroform, water, and glacial acetic acid* fulfilled all the necessary conditions; so that, when a mixture of equal weights of the first two with not too large a proportion of the third was well agitated and allowed to stand, it separated into two ternary solutions exactly correlative with the ternary alloys previously examined; the heavier one consisting chiefly of chloroform with some of the acetic acid and an amount of water proportionate to the acetic acid present; the other consisting mainly of water with the rest of the acetic acid, and more or less chloroform dissolved therein. Calling any given such pair of conjugate mixtures A and B respectively, we found that *the general principle deduced by Sir G. G. Stokes could be verified with sensible accuracy with these liquids*; on agitating together A and B in various proportions, *each liquid separated out again unchanged in each case, no matter whether A was used in large excess of B, or vice versa*. On the other hand, when two different alloys, A and B, were made of lead, tin, and zinc in such proportions that one was approximately conjugate to the other as indicated by the previously recorded observations, we did not succeed in getting anything like such sharp results; experiments where 2 parts of A to 1 of B were mixed together, and treated side by side with a mixture of 1 part of A to 2 of B, did not give quite the same results in the two cases, the differences being considerably larger than anything attributable to errors of analysis and such like sources of inaccuracy.

Mixtures of Chloroform, Water, and Acetic Acid.

The analysis of such mixtures we found could be carried out with considerable accuracy and ease in the following way; a weighed portion of the mixture (contained in a stoppered bottle) was diluted with water, and titrated with a fresh caustic soda solution accurately standardised, using phenolphthaleïn as indicator. Another portion, weighed in a flask or bulb tube containing a little water, was then submitted to the action of a current of dry air sucked through it, the issuing gases and vapours being made to pass through a pumice-stone and sulphuric acid drying tube. When constancy of weight was attained, and all chloroform had been removed, the loss of weight of the entire apparatus represented the chloroform; whilst the gain in weight of the apparatus (as compared with its weight before intro-

ducing the mixture) represented the water and acetic acid jointly, from which the water was obtainable by subtracting the weight of acetic acid deduced from the previous titration. A number of preliminary experiments showed that the sulphuric acid drying tube sufficed to retain all traces of acetic acid carried away by the current of air, whilst, on the other hand, it did not permanently absorb chloroform, and did not sensibly act on the chloroform so as to break it up, or hydrolyse it into hydrochloric and formic acids, &c.

As a first experiment, we thought it desirable to find out how short a time might be requisite to bring about such a condition of equilibrium (after vigorous agitation) that no sensible further alterations took place in the composition of the two liquids formed. We found that agitation for a minute or two at a time at intervals for a period of an hour always sufficed to bring about this state of matters. Thus, the following numbers were obtained in one set of observations, the liquids being contained in a well-stoppered stopcock-reservoir, so that the lower liquid could be readily sampled by opening the stopcock, and the upper one by means of a pipette. The original mixture contained—

Chloroform	30.0 per cent.
Water	29.7 "
Glacial acetic acid ($C_2H_4O_2$)	40.3 "
100.0	

	Bottom fluid.			Top fluid.		
	Chloroform.	Water.	Acetic acid.	Chloroform.	Water.	Acetic acid
Agitated at intervals for 1 hour: allowed to stand 1 hour more.	..	..	21.21	..	..	48.68
Next day (about 18 hours afterwards) ..	75.53	3.18	21.29	10.50	40.96	48.54
Agitated at intervals for a week: allowed to stand 2 hours since last agitation..	75.68	3.04	21.28	10.53	40.94	48.53

Analogous figures were obtained in several other similar experiments, the differences observed after different periods being but small (much less than 1 per cent.), and obviously due to experimental

errors, more especially slight variations of temperature; but with imperfectly intermixed fluids differences of much greater magnitude were often observed.

Next we prepared a series of mixtures containing as nearly as possible equal weights of water and chloroform with varying proportions of acetic acid up to 50 per cent. of the last. This amount produced a single homogeneous fluid not separating into two liquids, whereas with 45 per cent. separation readily occurred. The following average numbers were obtained from about twenty experiments:—

Percentage of acetic acid originally used.	Bottom fluid.			Top fluid.			Excess of percentage of acetic acid in top fluid over that in bottom.
	Chloro-form.	Water.	Acetic acid.	Chloro-form.	Water.	Acetic acid.	
0	99·01	0·99	0	0·84	99·16	0	..
4·0	98·24	0·72	1·04	0·92	92·62	6·46	5·42
10·9	94·98	1·19	3·83	0·79	81·52	17·69	13·86
16·5	91·85	1·38	6·77	1·21	73·69	25·10	18·33
19·2	91·23	0·82	7·95	1·85	70·42	27·73	19·78
24·6	87·82	1·13	11·05	2·97	63·32	33·71	22·66
35·2	80·00	2·28	17·72	7·30	48·58	44·12	26·40
42·6	72·86	3·62	23·52	12·82	37·82	49·36	25·84
44·9	70·13	4·12	25·75	15·11	34·71	50·18	24·43

These figures clearly show the close analogy between mixtures of chloroform, water, and acetic acid, and such ternary metallic mixtures as lead-zinc-tin, bismuth-zinc-silver, &c. On plotting curves as with the alloys previously described, the following results are deducible:—

1. Plotting percentages of acetic acid in one mixture (the lighter one, for example) as abscissæ, and excesses of percentages of acetic acid in top over those in bottom fluids as ordinates, the curve indicated in fig. 4 is obtained, closely resembling in general features those obtained with lead-zinc-silver and bismuth-zinc-silver alloys, the curve ascending to a maximum elevation and then coming partly down again, but not so much so as to descend again to the base line.
2. Percentages of acetic acid in heavier liquids as abscissæ, and those of water as ordinates. Curve shown in fig. 5, representing the *solubility of water in chloroform in presence of acetic acid*.

FIG. 4.

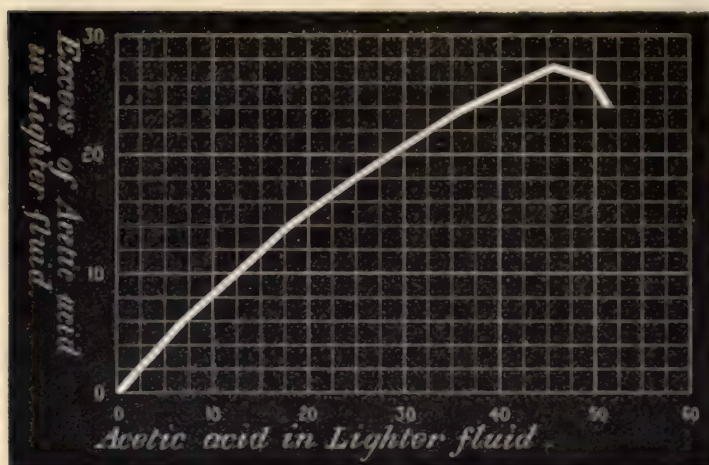
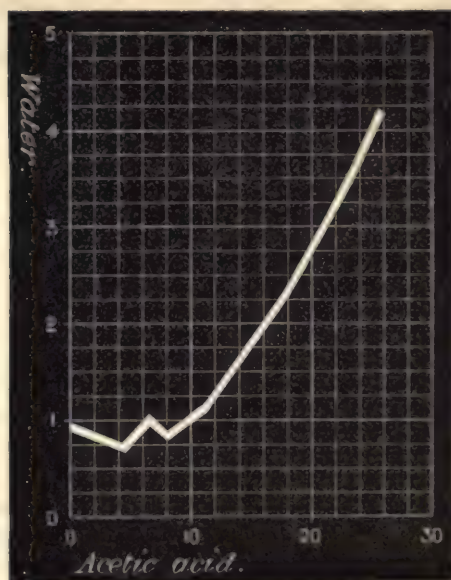
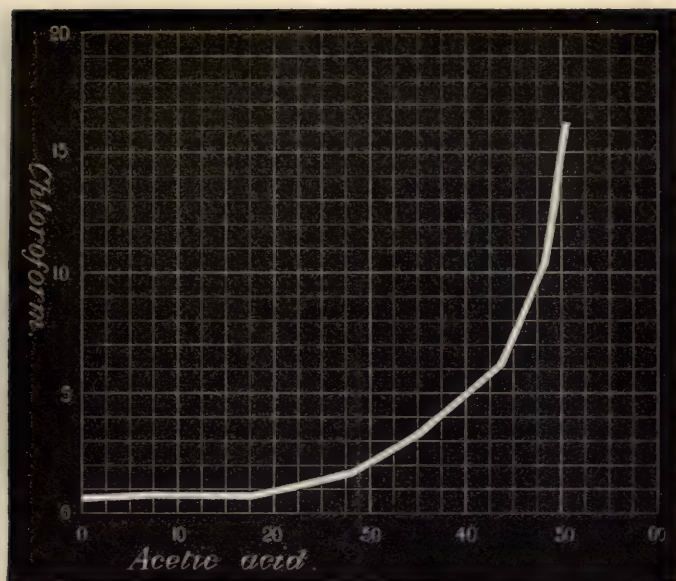


FIG. 5.



3. Percentages of acetic acid in lighter liquids as abscissæ, and those of chloroform as ordinates. Curve shown in fig. 6, representing the solubility of chloroform in water containing acetic acid.

FIG. 6.



These two solubility curves closely resemble those of the metals in general features, rising upwards at an accelerating rate, so that the curves are somewhat concave upwards.

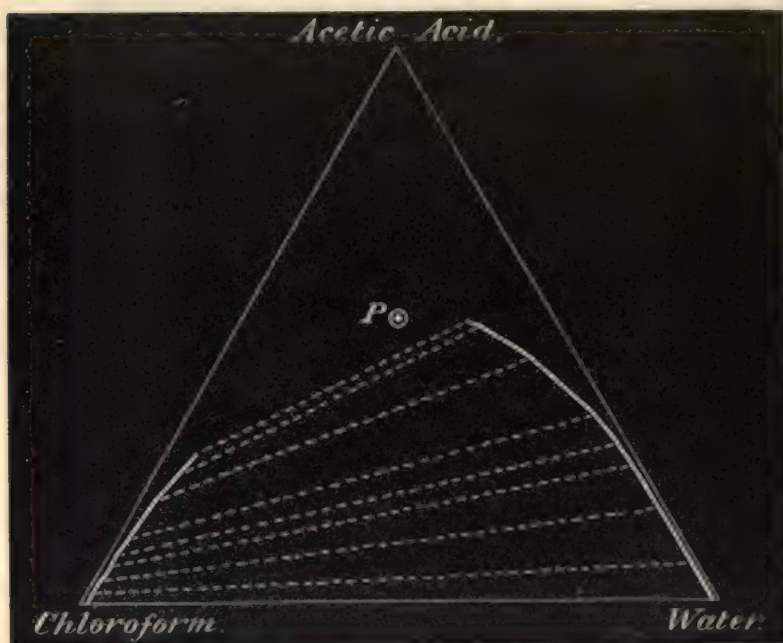
Fig. 7 shows the same results plotted in accordance with Sir G. G. Stokes's triangular method of graphical representation, the two branches of the critical curve being represented by the continuous lines, and the ties by the dotted lines.

The point P represents the mixture containing

Acetic acid	50 per cent.
Water	25 „
Chloroform	25 „

which, as above stated, was homogeneous, not separating into two different fluids; consequently P is a point *outside* of the space representing "ideal" mixtures bounded by the base line and the two branches of the critical curve. Just as in the case of fig. 2, the direction of slope of the ties obviously indicates that the lighter fluid always contained the larger proportion of acetic acid; but the variation in the difference between the proportions of acetic acid in the two fluids is not so clearly indicated as by the ordinary method of plotting shown in fig. 4, where the difference in acetic acid percentage between the two fluids visibly attains a maximum and then decreases.

FIG. 7.



We next prepared various mixtures of chloroform, water, and acetic acid in known proportions, agitated them thoroughly together, and, by means of a separating reservoir, drew off into separate vessels the heavier and lighter portions. Weighed quantities of these were then transferred to stoppered vessels, and again well agitated together at intervals for some time. After again separating by standing, samples of the heavier and lighter fluids formed were drawn off and analysed. The following figures were obtained in several such experiments:—

I. Equal weights of Chloroform and Water used, and Acetic Acid = 30 per cent. of the whole. The two conjugate mixtures formed were then agitated together in three different proportions, viz.:

- (α .) 1 part of heavier liquid to 3.0 of lighter.
 (β .) 1 " " 1.6 "
 (γ .) 1 " " 0.5 "

	Chloroform.		Water.		Acetic acid.	
	Heavy liquid.	Light.	Heavy.	Light.	Heavy.	Light.
α	84.44	4.15	1.54	56.22	14.02	39.63
β	84.57	4.00	1.32	56.22	14.11	39.78
γ	84.24	4.10	1.46	56.25	14.30	39.65

Obviously in all three cases the compositions of the heavy and light liquids respectively are sensibly identical.

II. Equal weights of Chloroform and Water, and Acetic Acid = 19.5 per cent. of the whole.

- (α .) 1 part of heavier liquid to 2.0 of lighter.
 (β .) 1 " " 1.33 "
 (γ .) 1 " " 0.5 "

	Chloroform.		Water.		Acetic acid.	
	Heavy liquid.	Light.	Heavy.	Light.	Heavy.	Light.
α	90.77	1.78	0.75	71.25	8.48	26.97
β	91.68	1.91	1.01	69.74	7.31	28.35
γ	91.23	1.87	0.70	70.24	8.07	27.89

III. Equal weights of Chloroform and Water, and Acetic Acid = 44.9 per cent. of the whole.

- (α .) 1 part of heavy liquid to 3.3 of lighter.
 (β .) 1 " " 0.49 "

	Chloroform.		Water.		Acetic acid.	
	Heavy liquid.	Light.	Heavy.	Light.	Heavy.	Light.
α	70.46	15.13	3.97	34.92	25.57	49.95
β	69.82	15.08	4.29	34.62	25.89	50.30

In neither of these experiments is there any difference in composition observable to an extent greater than might readily be supposed to be due to experimental errors, including those caused by differences in laboratory temperature at different times whilst making the observations.

In the following two experiments only the acetic acid was determined :—

Parts of lighter liquid to 1 of heavier.	Acetic acid.	
	Heavy liquid.	Light.
IV. { 2·33 0·56	21·28	48·53
	21·28	48·38
V. { 2·0 1·0 0·5	5·76	22·27
	5·44	22·77
	5·54	22·35

Experiments with Approximately Conjugate Alloys of Lead, Tin, and Zinc.

The experiments above described most strongly suggest that when interfering causes are removed, so that the mixtures of liquids dealt with can sensibly attain a condition of equilibrium, truly conjugate pairs of mixtures are formed, as supposed by Sir G. G. Stokes, of such a nature that the two may be intermixed in any proportions without any alteration in composition being thereby caused. Presumably the chief interfering cause in the former experiments with lead, zinc, and tin lay in the difficulty of obtaining thorough intermixture by simply stirring vigorously in a hot crucible; it might, therefore, be expected that if, instead of stirring together the three metals melted *en masse*, they were divided into two fractions and separately melted in such proportions as to produce two masses of approximately the composition of a pair of conjugate alloys, and these alloys were then mixed together and well stirred, a nearer approximation to truly conjugate compositions might be attained. We tried several experiments in this direction, but the results were far less sharp and well defined than those obtained with chloroform, water, and acetic acid, where a much more thorough intermixture by agitation in a closed vessel could be readily effected.

Thus, in one set of experiments we first prepared two alloys of approximately conjugate composition for a temperature of about 800° (Part II, 'Roy. Soc. Proc.,' vol. 48, p. 29), viz.:—

	Tin.	Lead.	Zinc.
Heavier alloy	29·5	50·0	20·5
Lighter alloy	28·5	13·0	58·5

Two parts of the first and one of the second were then melted in two separate crucibles, and the contents of one crucible poured into the other, and well intermixed by vigorous stirring for some minutes; the whole was then poured into a red-hot narrow clay crucible, and maintained at near 800° for 8 hours in the lead bath. Simultaneously, a second clay test-tube was heated, containing a similarly prepared mixture of one part of the first alloy to two of the second. The compound ingots ultimately obtained were analysed with the following results, obviously showing much less close agreement than in the case of the chloroform, water, and acetic acid; moreover, the difference in tin percentage between top and bottom underwent changes in opposite directions to extents closely commensurate with those calculable from the values deduced in Part I for the differences in the curves obtained according as lead or zinc predominated in the original mass, or as the two were present in equal proportions.

	Heavier end.			Lighter end.			Excess of tin percentage in lighter over that in heavier.
	Tin.	Lead.	Zinc.	Tin.	Lead.	Zinc.	
2 parts of first alloy to 1 of second.....	30·16	47·09	22·75	26·39	11·41	62·20	-3·77
2 parts of second alloy to 1 of first.....	28·05	52·34	19·61	28·76	11·69	59·55	+0·71
Difference ..	+2·11	-5·25	+3·14	-2·37	-0·28	+2·65	4·48

Similarly, in two other sets of experiments, the following tin percentages were obtained, again showing a notable divergence in the results according as the heavy alloy was employed to double the extent of the lighter one, or only half.

	Heavier end.	Lighter end.	Excess in lighter end.	Heavier end.	Lighter end.	Excess in lighter end.
2 parts of first heavier alloy to 1 of lighter.	15·39	17·51	+ 2·12	23·45	23·39	- 0·06
2 parts of first lighter alloy to 1 of heavier.	15·07	19·56	+ 4·49	22·57	25·41	+ 2·84
Difference.....	+ 0·32	- 2·05	2·37	+ 0·88	- 2·02	2·90

In every case the same general result is noticed, that when the two approximately conjugate alloys are intermixed in such proportions that lead predominates over zinc in the total mass, or *vice versa*, the differences in tin percentage between the two ends of the compound ingots formed are of the same kind as those observed in Part I with original mas-es containing lead and zinc in different ratios: viz., that when lead predominates a point is obtained belonging to a curve *underlying* that pertaining to cases where zinc predominates: whence it appears pretty certain that, whatever the causes may be that prevent truly conjugate alloys from being obtained under the conditions of the one set of experiments (whether incomplete intermixture, or something else), they also operate in the other series of observations.

Taking into account, however, the fact that in the experiments with chloroform, water, and acetic acid truly conjugate mixtures were obtained when a sufficient amount of intermixture by agitation had occurred, *but not till then*, the final conclusion appears to be warranted that the proposition set forth by Sir G. G. Stokes is a perfectly correct one, and that the divergences noticed in certain of the alloy experiments are due to the inherent nature of the case as regards the difficulties in the way of obtaining sufficiently complete intermixture: possibly these difficulties might be overcome by enclosing the fused mixtures of metals in a stepped vessel or crucible-flask of clay, and agitating this by long continued shaking about, whilst keeping it sufficiently hot in some kind of muffle furnace; but the appliances at our disposal have not permitted us actually to decide this point experimentally. The difficulty of carrying out such experiments is further enhanced by the circumstance that metallic alloys, when intermixed by vigorous agitation, do not appear to separate again from one another anything like so readily as such substances as chloroform and water or ether and water; small vesicles or droplets of the heavier alloy remain suspended in the lighter one (and *vice versa*) for long

periods of time, necessitating the maintenance of a nearly equable temperature, and the remaining at rest for many hours, before the top part of the mass becomes sensibly free from suspended portions of the heavier alloy, and the bottom part from similar portions of lighter alloy. The analytical numbers obtained on examining different layers of the compound ingots prepared in the experiments described in the earlier parts of these researches long ago convinced us of this; but, in addition, an actual visible presence of suspended particles of one alloy in the midst of another, even after 8 hours tranquil fusion, may be often observed in the case of silver-lead-zinc and silver-bismuth alloys where the proportions of metals used are such as to form mixtures containing considerable amounts of Ag_4Zn_5 : by the aid of a lens, or even with the naked eye, red particles disseminated through a much lighter coloured matrix can often be distinguished on examining the central portions of an ingot that has been filed smooth and bright, and then kept for awhile so as to allow the red tinge to develop.

IV. "On the Structure of Amœboid Protoplasm, with a Comparison between the Nature of the Contractile Process in Amœboid Cells and in Muscular Tissue, and a Suggestion regarding the Mechanism of Ciliary Action." By E. A. SCHÄFER, F.R.S. Received January 26, 1891.

It has been shown by the researches of numerous histologists, of whom Heitzmann and Frommann, and, in this country, Klein, must be reckoned the pioneers, that the protoplasm of many cells exhibits the appearance of a network containing an apparently homogeneous material in its meshes. The network is known as the *reticulum* or *spongioplasm*, the clear material in its meshes as *enchylema* (Carnoy) or *hyaloplasm*. In many cells it is not difficult to observe this structure even without the addition of reagents, but in amœboid cells such as the white blood corpuscle and the amœba it is less obvious, and its presence has not been generally conceded. Recently, Professor Stricker* has published a photograph of an amœboid white blood corpuscle, taken instantaneously by aid of the electric light, which shows the reticular appearance in quite an unmistakable manner; it must be granted, therefore, that the amœboid white blood corpuscle also has this structure.

Previously to the appearance of Professor Stricker's photograph, I had myself for some time been engaged in investigating the structure of amœboid cells with the aid of photography. Being unprovided

* 'Wiener Medic. Jahrb.,' 1890.

with the appliances necessary for photographing by the electric light. I was unable to obtain instantaneous photographs, and could not photograph the corpuscles while actually living and moving. I accordingly adopted a method of suddenly killing the corpuscles whilst still in the amœboid condition with their pseudopodia extended. It is well known that with most methods which are employed to fix the white blood corpuscles there is time for a contraction of the protoplasm to be produced, so that the pseudopodia are withdrawn and the corpuscle becomes spherical. The method which I have used consists in the instantaneous application of a jet of steam to the surface of the cover-glass. A preparation of blood, preferably from the newt (*Triton cristatus*), is made either in a moist chamber or in the usual way on a glass slide. In a short time the white corpuscles become highly amœboid and throw out pseudopodia, which may spread themselves in a thin layer upon the cover glass in a manner which is perfectly adapted for their being accurately observed. If the steam be now turned on for an instant, the cells are suddenly killed, and remain exactly in the condition in which they happened to be when the heat was applied. They can be examined and photographed thus, or may first be stained by hæmatoxylin, with or without being previously treated with alcohol. In all cases they exhibit the same general structural appearances, and these appearances can even be detected, but with greater difficulty, in the cell whilst still living.

Leaving the nucleus, which beautifully exhibits the karyoplasmic network, out of consideration, the most striking point in all amœboid white corpuscles thus prepared is the contrast between the protoplasm of the body of the cell and that of the pseudopodia. For whilst the former exhibits, according to focus, either a finely punctated or a reticular aspect, and stains decidedly with hæmatoxylin, the pseudopodia exhibit not the faintest trace of structure, and remain almost entirely unstained.

In other words, the protoplasm is composed of two morphologically distinct parts, one which exhibits a reticular arrangement and has an affinity for hæmatoxylin, and another which shows to the best optical appliances no structural arrangement, and is also chemically different, as is shown by its behaviour to staining reagents.

The observation here recorded is not an isolated one. Almost all observers who have given special attention to the matter have failed to detect a reticular structure in pseudopodia, whether of the amœboid cells of higher organisms or of the Rhizopoda. To Bütschli's theory of the structure and activity of protoplasm,* whereby he endeavours to show that the reticular appearance and amœboid phenomena may be explained on the assumption that protoplasm is

* 'Heidelberg Verhandlungen,' 1890; and 'Biologisches Centralblatt,' 1890.

not an actual network with enchylema, but rather a frothy mixture of two dissimilar substances, this absence of all apparent structure in pseudopodia offers an admittedly serious difficulty, which he endeavours to surmount by assuming that the same frothy structure is really present in the pseudopodia as in the body of the cell, but that owing to thinning out it cannot be detected. But apart from the unlikelihood of our not noticing such structure in the pseudopodia if it were really present, since they are especially well adapted for minute observation, the reticular and the homogeneous substances should, according to this assumption, pass gradually the one into the other, for the thinning-off of the pseudopodia is frequently gradual. The contrary is, however, the case. The line of demarcation of the reticular substance is always quite sharp, and does not thin off into the homogeneous substance of the pseudopodia.

Stricker's photograph is also really evidence in the same direction. The corpuscle taken is spherical or nearly so, *i.e.*, is in the contracted condition. It has, however, one small pseudopodium. This is absolutely without structure; it is the spherical part of the cell which shows the reticulum.

It is well known that if white corpuscles (and contracted amœboid cells generally) are artificially stimulated, they are always spherical. The spherical form is, in fact, the contracted condition; it is only in the absence of any obvious source of excitation that the corpuscle throws out pseudopodia. The spherical condition is immediately produced by electrical or mechanical stimuli; no doubt, the constant mechanical stimulation which the cells receive in the circulating blood maintains them in the spherical form which they always exhibit whilst moving within the blood-vessels. Possibly, also, the contact of a foreign particle, causing the contraction and withdrawal of the protoplasm which it touches, and the consequent inception of the particle, is another instance of mechanical stimulation.

Now, in the contracted corpuscle, the whole cell appears reticular, and the reticulation is even better marked, *i.e.*, coarser, than that seen in the spread out corpuscle. The pseudopodial protoplasm or hyaloplasm has, in fact, been withdrawn into the meshes of the framework or spongioplasm.

The protoplasm of such an amœboid cell as the white blood corpuscle may, therefore, be regarded as composed of two distinct substances, spongioplasm and hyaloplasm. Spongioplasm has a reticular or sponge-like arrangement, an affinity for staining fluids, is firmer than the hyaloplasm (but, perhaps, not actually solid), and is, in all probability, highly extensile and elastic. Hyaloplasm, on the other hand, is structureless, has little or no affinity for stains, and is highly labile and fluent. It is by the active flowing of the hyalo-

plasm, not by the contraction of the spongioplasm (as conceived by Carnoy*), that the movements of cells are produced.† Of the two substances, the hyaloplasm is the more active, the spongioplasm the more inert. The spongioplasm forms, in fact, a sort of framework supporting the hyaloplasm, and into which under the influence of stimuli the hyaloplasm becomes wholly withdrawn. To adopt Bruecke's well-known terminology, the hyaloplasm is the *zoid*, the spongioplasm its *ecoid*.

Whether one or other of these two substances is ever wholly absent from the protoplasm of cells is a question which cannot at present be decided. There are cells and unicellular organisms, both animal and vegetable, in which no reticular structure can be made out, and these *may* be formed of hyaloplasm alone. In that case, this must be looked upon as the essential part of protoplasm. So far as amœboid phenomena are concerned, it is certainly so; but whether the chemical changes which occur in many cells are effected by this or by spongioplasm is another question. Certainly, the reticulum is always very well marked in cells in which considerable chemical changes are produced, *e.g.*, gland cells.

The movements within plant cells must also be regarded as due to the flowing of hyaloplasm. It is, indeed, impossible to conceive that the contraction of a reticulum could produce the circulation of the protoplasm which is seen within a cell of *Vallisneria*. How the flowing is produced is an entirely different question, and one which must at present remain unanswered.

If now we compare the structure of protoplasm with that of striated muscle, we find many points of coincidence. As is well known, the muscle columns of the wing muscles of insects ("wing-fibrils" of authors) are divided by transverse partitions (membranes of Krause) into a series of segments (sarcomeres, Muskel-kästchen of Krause), each of which contains a sarcous element or disk of anisotropic sarcous substance (which is really formed of two halves, their junction being often visible as the line of Hensen), and a homogeneous isotropic substance, which in the extended muscle occupies the intervals between the sarcous element and the transverse membrane. As I have elsewhere recently shown,‡ the substance of the sarcous element is penetrated by pores or canals which extend in each half of the element as far as the line or plane of Hensen, and which are occupied by clear substance continuous with the homogeneous substance of the intervals. The substance of the sarcous element stains with hæmatoxylin and similar reagents, while the homogeneous substance of the clear intervals remains unstained. When the

* 'Biologie Cellulaire,' 1886.

† Cf. Leydig, 'Zelle u. Gewebe,' Bonn, 1885.

‡ 'Monthly International Journal of Anatomy and Physiology,' vol. 8, 1891.

muscle contracts, the homogeneous substance passes from the intervals into the pores of the sarcous element, and thus enlarges the latter, while the clear intervals are proportionately shortened, so that in extreme contraction they may disappear, and the swollen and bulged sarcous element may almost abut against the transverse membranes. On the other hand, when the contraction passes off, and the muscle becomes extended, the homogeneous substance passes out of the pores of the sarcous element into the clear intervals; the latter become manifest, and the sarcous element proportionately diminished in bulk. It is hardly possible that the resemblance of these changes to those which occur in the protoplasm of an amœboid cell is merely accidental—difficult not to believe that the perforated sarcous substance is the spongioplasmic “œcoid,” the clear labile substance the hyaloplasmic “zoid.”

This conception of the structure and mode of activity of the amœboid cell and of muscle, whilst bringing them under exactly the same category, and thus tending to simplify our ideas regarding contractile phenomena, may also serve to aid in the elucidation of certain questions in connection with those phenomena which have long presented difficulties to the physiologist and pharmacologist. For example, with regard to the movements of amœboid cells, the question has been frequently discussed, and never satisfactorily answered, whether we are to regard the withdrawal of the pseudopodia into the body of the cell as the condition of rest, and the protrusion of the pseudopodia as the condition of activity, or *vice versâ*. Viewed by the light of the above observations, it is clear that neither state is to be regarded as a resting condition; both are manifestations of activity; both are produced by flowing of the hyaloplasm. Similarly, in the case of muscle, the passage from the contracted to the extended condition can no longer, as is so frequently assumed, be looked upon as a merely passive change of state, but must be regarded, no less than in the case of the passage from the extended to the contracted condition, as produced by flowing of hyaloplasm. In the one case this flows *into* pores of the spongioplasm—this is the condition called contraction, and ordinarily regarded as the active state; in the other case there is a flowing of the hyaloplasm *out* of the pores of the spongioplasm, by which movement the condition of extension is determined. That different chemical and electrical changes accompany, perhaps determine, these different directions of movement is well known. It is also known that the process of extension is influenced by drugs, independently of the action they may exert upon that of contraction (Brunton, Ringer). But whether the chemical and electrical changes, and those produced by drugs, occur in the hyaloplasm, or in the spongioplasm, or in both substances, is a question which, as in the analogous case of the amœboid cell, cannot

at present be decided. The same remark may be made with respect to the question of active participation by the spongioplasm in the production of the movements of the hyaloplasm. It is, however, quite certain from the observation of the movements of the hyaloplasm of pseudopodia, which may actively flow in different directions, even when far removed from the spongioplasm, that it is the hyaloplasm which is to be regarded as the physically active part of protoplasm, and therefore also presumably of muscular substance.

Lastly, there is another form of protoplasmic activity, viz., ciliary motion, which cannot be left out of consideration in any attempt to explain the manner in which the contractile manifestations of protoplasm are produced. On this matter I have no new facts to record, and the suggestion therefore that I have to make must be understood to be a purely theoretical deduction from analogy, and not based upon actual observation. At the same time it does not, so far as I know, stand in contradiction to any known fact. The suggestion is briefly this:—If we suppose that a cilium is a hollow curved extension of the cell, occupied by hyaloplasm, and invested by a delicate elastic membrane, then it must follow that if there be a rhythmic flowing of hyaloplasm from the body of the cell, into and out of the cilium, an alternate extension and flexion of that process would thereby be brought about. The movement would in fact be produced by an action which would be practically the same as that by which the amœboid movements of cells and the contraction and extension of muscle are probably effected. The same result might be got, supposing the cilium to be a straight and not a curved extension of the cell, if the enveloping membrane were thicker (or otherwise less extensible) along one side than along the other. This assumption would also enable one better to account for the spiral direction of the movement of certain cilia; for this form of movement would be produced if the line of lessened extensibility in them were to pass in a corkscrew fashion along the cilium in place of straight along one side, as might be assumed for ordinary cilia.

V. "On the Demonstration by Staining of the Pathogenic Fungus of Malaria, its Artificial Cultivation, and the Results of Inoculation of the same." By Surgeon J. FENTON EVANS, M.B. Communicated by Professor VICTOR HORSLEY, F.R.S. (From the Laboratory of the Brown Institution.) Received February 7, 1891.

(Abstract.)

The discovery of organisms constantly concomitant with manifestations of malaria was made by Laveran in 1880.

His researches have since been corroborated and amplified by numerous observers in different parts of the world, among whom must be mentioned, Marchiafava, Celli, Golgi, and Guarneri, in Italy; Councilman, Osler, and James, in America; and Vandyke Carter, in India. The foreign structures which all of the above-named investigators agree in finding in the blood during or after attacks of ague may be grouped into the following classes:—

1. "Cystic" bodies or spores, 2 to 11 μ in diameter, round, transparent, encapsuled bodies of variable dimensions.
2. Crescentic bodies, 8 to 9 μ long and 3 μ broad.
3. Plasmodia malarie, organisms as variable in size as the "cystic" bodies or spores, possessing the power of amœboid movement, and so closely associated with the red blood corpuscle that hitherto the majority of observers have considered them to be parasites situated within the red blood cells.
4. Mobile filaments, 21 to 28 μ long.

Despite the general concord of the observations, the subject has not advanced beyond the stage of recognition of these structures in the blood, and that, too, only while in the fresh state.

No method had hitherto been discovered of preparing permanently stained specimens of the organism.

It had never been isolated or classified, nor when thus separated had its pathogenic qualities ever been tested by experiments on lower animals.

It was thus clear that much remained to be done, and in the paper are recounted the attempts made to place the subject on a satisfactory footing. The author has found that it is possible to stain the organisms with an anilined alkalised solution of rosanilin hydrochloride after treatment with bichromate of potash, and after treatment with dilute sulphuric acid by an anilined alkalised solution of Weigert's acid fuchsin.

Another method of staining consisted in the saturation of the tissue with a copper salt and its reduction by sulphuretted hydrogen previous to coloration with anilised alkalised acid fuchsin.

By these staining methods the organisms have been demonstrated in the blood, and also in the tissues. And some new, hitherto unrecognised features are described, among which may be mentioned what appears to be the germination of the spore in the blood, the existence of a comma-shaped body and of mycelium in the spleen and Peyer's glands, and the localisation of the plasmode, *i.e.*, in relation to the blood corpuscles.

The isolation of the organism and its artificial cultivation have been successfully carried out, and it is shown that this result entirely depends for its success upon the fact that the nutrient media must be previously treated with living blood, *i.e.*, before rigor mortis has set in.

Alteration in the chemical composition of the nutrient medium, consisting in the addition of glucose, together with iron or hæmoglobin or fresh blood, to the non-peptonised beef broth, elicited the interesting fact that, under these circumstances, the organism can pass to a more highly developed state, displaying the structure and fructification of a highly organised fungus, but differing in certain important features from any fungus hitherto described.

Inoculation of guinea pigs, monkeys, and rabbits with the growths in various nutrient media has produced a frequently fatal disease, which, although not characterised in these animals by the symptoms of classical intermittent fever, yet displayed in a number of instances a definitely intermittent character. It was further, whatever its clinical character, invariably accompanied by the appearance of the characteristic organisms in the blood drawn after death from the right ventricle.

It is accordingly concluded that the malarial fungus is capable of being cultivated outside the body and has been proved to possess pathogenic qualities.

Presents, February 12, 1891.

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February 19, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

- I. "On the Sensitiveness of the Bridge Method in its Application to Periodic Electric Currents." By LORD RAYLEIGH, Sec. R.S. Received January 17, 1891.

The most favourable conditions in the ordinary measurement of resistance have been investigated by Schwendler* and by O. Heaviside.† It is here proposed to treat the problem more generally, so as to cover the application to conductors endowed with self-induction, or combined with condensers. The receiving instrument may be supposed to be a telephone, which takes the place of the galvanometer

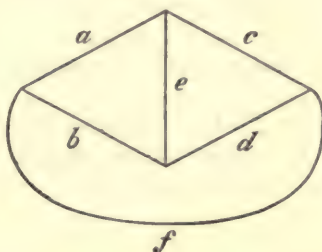
* "On the Galvanometric Resistance to be employed in Testing with Wheatstone's Diagram," 'Phil. Mag.,' vol. 31, p. 364, 1866.

† "On the Best Arrangement of Wheatstone's Bridge for measuring a given Resistance with a given Galvanometer and Battery," 'Phil. Mag.,' vol. 45, p. 114, 1873.

employed in ordinary testing. In the conjugate "battery" branch a periodic electromotive force of given frequency is the origin of the currents.

Special attention will be given to the case where the branches are equal in pairs, *e.g.*, $a = c$, $b = d$ (fig. 1). The advantages of this arrangement are important even in ordinary resistance testing, and in the generalised application are still more to be insisted upon. By mere interchange of a and c and combination of results, the equality of b and d can be verified independently of the exactitude of the ratio $a : c$.

FIG. 1.



If any element in the combination, for example a , be a mere resistance, the difference of potentials at its terminals (V) is connected with the current, x , by the relation

$$V = ax.$$

We have, however, to suppose that a is not merely a resistance or even combination of such. It may include an electromagnet,* and it may be interrupted by a condenser. So long as the current is strictly harmonic, proportional to e^{ipt} , the most general possible relation between V and x is expressed by

$$V = (a_1 + ia_2)x,$$

where a_1 and ia_2 are the real and imaginary parts of a complex coefficient a , and are functions of the frequency $p/2\pi$. In the particular case of a simple conductor, endowed with inductance L , a_1 represents the resistance, and a_2 is equal to pL . In general, a_1 is positive; but a_2 may be either positive, as in the above example, or negative. The latter case arises when a resistance, R , is interrupted by a condenser of capacity C . Here $a_1 = R$, $a_2 = -1/pC$. If there be also inductance L ,

$$a_1 = R, \quad a_2 = pL - 1/pC.$$

* An electromagnet here denotes a conductor with sensible inductance. Iron may be present if the range of magnetisation be small.—'Phil. Mag.,' March, 1887.

Since the parts of a_2 may be either positive or negative, there is nothing to hinder its evanescence by compensation. In the above combination of an electromagnet and condenser compensation occurs when $p^2LC = 1$, that is, when the natural period with terminals connected coincides with the forced period. The combination is then equivalent to a simple resistance;* but a variation of frequency will give rise to a positive or negative a_2 .

The case of two electromagnets in parallel is treated in my paper on "Forced Harmonic Oscillations;"† and other combinations have been discussed by Mr. Heaviside and myself. But the above examples will suffice to illustrate the principle that the relation of V to x is one of proportionality, and may be expressed by the single complex symbol a . We fall back at any time upon the case of mere resistance by supposing a to be real. In like manner b, c, d, e , and f are symbols expressing the electrical properties of the remaining branches.

In all electrical problems the generalised quantities a, b , &c., combine, just as they do when they represent simple resistances. Thus, if a, a' be two complex quantities representing two conductors in series, the corresponding quantity for the combination is $a + a'$. Again, if a, a' represent two conductors in parallel, the reciprocal of the resultant is given by addition of the reciprocals of a, a' . For, if the currents be x and x' , corresponding to a difference of potentials V at the common terminals,

$$V = ax = a'x',$$

so that

$$x + x' = V(1/a + 1/a').$$

The investigation of the currents in networks of conductors is usually treated by "Kirchhoff's rules," and this procedure may of course be adopted in the present case to determine the current through the bridge of a Wheatstone combination. But it will be more instructive to put the argument in the form applicable to the forced vibrations of all mechanical systems which oscillate about a configuration of equilibrium.

If $p/2\pi$ represent the frequency of the vibration, the coordinates $\psi_1, \psi_2, \psi_3, \dots$ determining the condition of the system, and the corresponding forces $\Psi_1, \Psi_2, \Psi_3, \dots$ are all proportional to e^{ipt} , and the coordinates are linear functions of the forces.‡ For the present purpose we suppose that all the forces vanish, except the first and second. Thus ψ_1, ψ_2 are linear functions of Ψ_1 and Ψ_2 , and, conversely, Ψ_1, Ψ_2 may be regarded as linear functions of ψ_1 and ψ_2 . We may therefore set

* 'Theory of Sound,' § 46, Macmillan, 1877.

† 'Phil. Mag.,' May, 1886.

‡ 'Theory of Sound,' vol. 1, § 107.

$$\left. \begin{aligned} \Psi_1 &= A\psi_1 + B\psi_2 \\ \Psi_2 &= B\psi_1 + C\psi_2 \end{aligned} \right\} \dots\dots\dots (1),$$

the coefficient of ψ_2 in the first equation being identical with that of ψ_1 in the second by the reciprocal property. The three constants A, B, C are in general complex quantities, functions of p .

In the application that we have to make of these equations, $\psi_1, \psi_2, \Psi_1, \Psi_2$ will represent respectively currents and electromotive forces in the battery and telephone branches of the combination. The reciprocal property may then be interpreted as follows:—If $\Psi_2 = 0$,

$$B\psi_1 + C\psi_2 = 0,$$

and

$$\psi_2 = \frac{B}{B^2 - AC} \Psi_1 \dots\dots\dots (2).$$

In like manner, if we had supposed $\Psi_1 = 0$, we should have found

$$\psi_1 = \frac{B}{B^2 - AC} \Psi_2 \dots\dots\dots (3),$$

showing that the ratio of the current in one branch to an electromotive force operative in the other is independent of the way in which the parts are assigned to the two branches.

We have now to determine the constants A, B, C in terms of the electrical properties of the system. If ψ_2 be maintained zero by a suitable force Ψ_2 , the relation between ψ_1 and Ψ_1 is $\Psi_1 = A\psi_1$. In our application, A therefore denotes the (generalised) resistance to an electromotive force in the battery branch, *when the telephone branch is open*. This resistance is made up of f , the resistance in the battery branch, and of that of the conductors $a+c, b+d$ combined in parallel. Thus,

$$A = f + \frac{(a+c)(b+d)}{a+b+c+d} \dots\dots\dots (4).$$

In like manner,

$$C = e + \frac{(a+b)(c+d)}{a+b+c+d} \dots\dots\dots (4').$$

To determine B let us consider the force Ψ_2 which must act in e in order that the current through it (ψ_2) may be zero, in spite of the operation of Ψ_1 . We have $\Psi_2 = B\psi_1$. The total current ψ_1 flows partly along the branch $a+c$, and partly along $b+d$. The current through $a+c$ is

$$\frac{1}{\frac{1}{a+c} + \frac{1}{b+d}} \psi_1 = \frac{(b+d)\psi_1}{a+b+c+d} \dots\dots\dots (5),$$

and that through $b+d$ is

$$\frac{(a+c) \psi_1}{a+b+c+d} \dots\dots\dots (6).$$

The difference of potentials at the terminals of e , supposed to be interrupted, is thus

$$\frac{c(b+d) \psi_1 - d(a+c) \psi_1}{a+b+c+d};$$

or

$$B = \frac{bc-ad}{a+b+c+d} \dots\dots\dots (7).$$

By (4), (4'), (7) the relationship of Ψ_1, Ψ_2 to ψ_1, ψ_2 is completely determined.

The problem of the bridge requires the determination of the current ψ_2 , as proportional to Ψ_1 , when $\Psi_2 = 0$, that is, when no electromotive force acts in the bridge itself, and the solution is given at once by simple introduction into (2) of the values A, C, B from (4), (4'), (7).

If there be an approximate balance, the expression simplifies. For $bc-ad$ is then small, and B^2 may be neglected relatively to AC in the denominator of (2). Thus, as a sufficient approximation in this case, we have

$$\psi_2/\Psi_1 = \frac{\frac{ad-bc}{a+b+c+d}}{\left\{ e + \frac{(a+b)(c+d)}{a+b+c+d} \right\} \left\{ f + \frac{(a+c)(b+d)}{a+b+c+d} \right\}} \dots\dots (8),$$

in agreement with the equation used by Mr. Heaviside for simple resistances.

The following interpretation of the process leads very simply to the approximate form (8), and may be acceptable to readers less familiar with the general method. Let us first inquire what E.M.F. is necessary in the telephone branch to stop the current through it. If such a force acts, the conditions are, externally, the same as if the branch were open, and the current ψ_1 in the battery branch due to an E.M.F. equal to Ψ_1 in that branch is Ψ_1/A , where A is written for brevity as representing the right-hand member of (4). The difference of potential at the terminals of e , still supposed to be open, is found at once when ψ_1 is known. It is equal to

$$c \times (5) - d \times (6) = B\psi_1,$$

where B is defined by (7). In terms of Ψ_1 the difference of potentials is thus $B\Psi_1/A$. If e be now closed, the same fraction expresses the E.M.F. necessary in e in order to prevent the generation of a current in that branch.

The case that we have to deal with is when Ψ_1 acts in f , and there is no E.M.F. in e . We are at liberty, however, to suppose that two opposite forces, each of magnitude $B\Psi_1/A$, acts in e . One of these, as we have seen, acting in conjunction with Ψ_1 in f , gives no current in e ; so that, since electromotive forces act independently of one another, the actual current in e , closed without internal E.M.F., is simply that due to the other component. The question is thus reduced to the determination of the current in e due to a given E.M.F. in that branch.

So far the argument is rigorous; but we will now suppose that we have to deal with an approximate balance. In this case an E.M.F. in e gives rise to very little current in f , and in calculating the current in e we may suppose f to be broken. The total resistance to the force in e is then given simply by C of equation (4'), and the approximate value for ψ_2 is derived by dividing $-B\Psi_1/A$ by C , as we found in (8).

A continued application of the foregoing process gives ψ_2/Ψ_1 in the form of an infinite geometric series:—

$$\psi_2/\Psi_1 = -\frac{B}{AC} \left\{ 1 + \frac{B^2}{AC} + \frac{B^4}{A^2C^2} + \dots \right\} = \frac{B}{B^2 - AC} \dots (2).$$

This is the rigorous solution already found; but the first term of the series suffices for practical purposes.

The form of (8) enables us at once to compare the effects of increments of resistance and inductance in disturbing a balance. For let $ad = bc$, and then change d to $d + d'$ where $d' = d'_1 + id'_2$. The value of ψ_2/Ψ_1 is proportional to d' , and the amplitude of the vibratory current in the bridge is proportional to $\text{Mod } d'$, that is, to $\sqrt{(d'_1)^2 + (d'_2)^2}$. Thus d'_1, d'_2 are equally efficacious when numerically equal.

The next application that we shall make of (8) is to the generalised form of Schwendler's problem. When all else is given, how should the telephone, or other receiving instrument, be wound in order to get the greatest effect?

If by separation of real and imaginary parts we set

$$e = e_1 + ie_2, \quad \frac{(a+b)(c+d)}{a+b+c+d} = r_1 + ir_2 \dots\dots (9),$$

the factor in the denominator of (6) with which we are concerned becomes

$$e_1 + r_1 + i(e_2 + r_2);$$

and the square of the modulus is given by

$$\text{Mod}^2 = (e_1 + r_1)^2 + (e_2 + r_2)^2 \dots\dots\dots (10).$$

In this equation e_1, r_1 are essentially positive, while e_2, r_2 may be either positive or negative. If e_1 and e_2 are both at disposal, the minimum of (10), corresponding to the maximum current, is found by making

$$e_1 = 0, \qquad e_2 = -r_2 \dots\dots\dots (11).$$

But this is not the practical question. As in the case of simple resistances, what we have to aim at is not to render the current in the bridge a maximum, but rather the *effect* of the current. Whether the receiving instrument be a galvanometer or a telephone, we cannot in practice reduce its resistance to zero without at the same time nullifying the effect desired. We must rather regard the space available for the windings as given, and merely inquire how it may best be utilised. Now the effect required to be exalted is, *cæteris paribus*, proportional to the number of windings (m); and, if the space occupied by insulation be proportional to that occupied by copper, the resistance varies as m^2 . So also does the inductance; and accordingly, if the instrument be connected to the bridge by leads sensibly devoid of resistance and inductance,

$$e_1 + ie_2 = m^2 (\epsilon_1 + i\epsilon_2) \dots\dots\dots (12),$$

where ϵ_1, ϵ_2 are independent of m . The quantity whose modulus is to be made a minimum by variation of m is thus

$$\frac{e_1 + ie_2 + r_1 + ir_2}{m} = \frac{r_1 + m^2\epsilon_1 + i(r_2 + m^2\epsilon_2)}{m};$$

and we have

$$\begin{aligned} \text{Mod}^2 &= \frac{(r_1 + m^2\epsilon_1)^2 + (r_2 + m^2\epsilon_2)^2}{m^2} \\ &= (r_1^2 + r_2^2) m^{-2} + 2(r_1\epsilon_1 + r_2\epsilon_2) + (\epsilon_1^2 + \epsilon_2^2) m^2. \end{aligned}$$

This is a minimum by variation of m when

$$m^4 = \frac{r_1^2 + r_2^2}{\epsilon_1^2 + \epsilon_2^2},$$

or
$$\text{Mod}(r_1 + ir_2) = \text{Mod}(e_1 + ie_2) \dots\dots\dots (13).$$

We may express this result by saying that to get the best effect the instrument must be so wound that its *impedance* is equal to that of the compound conductor $r_1 + ir_2$. If for any reason the inductances can be omitted from consideration, then the resistance of the instrument is to be made equal to r_1 , in accordance with Schwendler's rule.

The case of the "battery" branch may often be treated in like manner. As Mr. Heaviside has shown, if a number of cells are

available for ordinary resistance testing, they should be combined, so that their resistance is equal to that (s_1) of the corresponding combination of wires in parallel. Periodic currents may be conceived to arise from the rotation of a coil in a magnetic field of given strength. If the space occupied by the windings of the coil be supposed to be given, their number m will be determined by the condition of equal impedances. Thus, if

$$\frac{(a+c)(b+d)}{a+b+c+d} = s_1 + is_2 \dots\dots\dots (14),$$

$$\text{Mod}(f_1 + if_2) = \text{Mod}(s_1 + is_2) \dots\dots\dots (15),$$

in analogy with (13).

The above is the solution of the problem, if the coils of the sending and receiving instruments represent the whole of their respective branches, and are limited to occupy given spaces. The inductances and resistances cannot then be varied independently. But there would often be no difficulty in escaping from this limitation. The inclusion of additional resistance, external to the instrument, can only do harm; but the case is otherwise with inductance, positive or negative. If the inductance of the instrument added to r_2 , or to s_2 , be positive, the total inductance may be reduced to zero by the insertion of a suitable condenser, and this without material increase of resistance. If the inductance be already negative, the remedy is not so easily carried out; but, theoretically, it is possible to add the necessary inductance without sensible increase of resistance. The greater the frequency of vibration, the more feasible does this course become. We may, therefore, without much violence, suppose that the inductances of two branches can be reduced to zero without additional resistance. Thus,

$$e_2 + r_2 = 0, \quad f_2 + s_2 = 0 \dots\dots\dots (16);$$

and the condition of maximum efficiency of the transmitting and receiving coils is then given by Schwendler's rule,

$$e_1 = r_1, \quad f_1 = s_1 \dots\dots\dots (17).$$

These suppositions form a reasonable basis for further investigation; but conclusions founded upon them will be subject to re-examination, especially in extreme cases. We may also now introduce the promised simplification,

$$a = c, \quad b = d \dots\dots\dots (18),$$

in accordance with which (8) becomes

$$\psi_2/\Psi_1 = \frac{d-b}{4b} \frac{2ab/(a+b)}{\{e + \frac{1}{2}(a+b)\} \{f + 2ab/(a+b)\}} \dots\dots (19).$$

Also $r_1 + ir_2 = \frac{1}{2} (a + b) = \frac{1}{2} (a_1 + b_1) + \frac{1}{2} i (a_2 + b_2) \dots (20).$

$$s_1 + is_2 = \frac{2 (a_1 + ia_2) (b_1 + ib_2)}{a_1 + ia_2 + b_1 + ib_2}$$

$$= 2 \frac{(a_1 + b_1) (a_1 b_1 - a_2 b_2) + (a_2 + b_2) (a_2 b_1 + a_1 b_2)}{(a_1 + b_1)^2 + (a_2 + b_2)^2}$$

$$+ 2i \frac{(a_1 + b_1) (a_2 b_1 + a_1 b_2) - (a_2 + b_2) (a_1 b_1 - a_2 b_2)}{(a_1 + b_1)^2 + (a_2 + b_2)^2} \dots (21).$$

It may be well to examine, first, the consequences of (19), in the case of simple resistances. Here

$$r_1 = \frac{1}{2} (a_1 + b_1), \quad r_2 = 0 \dots \dots \dots (22);$$

$$s_1 = 2a_1 b_1 / (a_1 + b_1), \quad s_2 = 0 \dots \dots \dots (23).$$

In accordance with the plan proposed, we are to make $e_2 = 0$, $f_2 = 0$;* $e_1 = r$, $f_1 = s_1$. Our equation then becomes

$$\psi_2 / \Psi_1 = \frac{d_1 - b_1}{8b_1 (a_1 + b_1)} \dots \dots \dots (24).$$

Here a_1 is still at disposal, and we see that according to (24) it ought to be diminished without limit. This conclusion does not harmonize with one obtained by Mr. Heaviside.† It must be observed, however, that $a_1 = 0$ is unpractical, involving, as it does, $s_1 = 0$, $f_1 = 0$. Even according to (24) there is little to be gained by diminishing a_1 below, say, $\frac{1}{2} b_1$. In this case

$$a_1 = \frac{1}{2} b_1, \quad e_1 = r_1 = \frac{3}{4} b_1, \quad f_1 = s_1 = \frac{3}{8} b_1 \dots (25).$$

Such an arrangement as (25) may be recommended for practical use.

When b_1 is large, there may be advantage in taking a_1 relatively smaller than in the above example. In such cases we approach the limiting condition of things, and have approximately

$$e_1 = r_1 = \frac{1}{2} b_1, \quad f_1 = s_1 = 2a_1 \dots \dots \dots (26),$$

$$\psi_2 / \Psi_1 = \frac{d_1 - b_1}{8b_1^2} \dots \dots \dots (27).$$

And the smallness of f_1 in comparison with b_1 may sometimes be a convenience.

* These conditions require no attention in galvanometric testing with steady currents, being satisfied by $p = 0$, independently of the nature of the instrument.
† *Loc. cit.*, p. 120, "In conclusion, if, to measure a certain resistance, the best resistances for the galvanometer, battery, and the three sides, a , b , c , were required, then we should have to make $a = b = c = d = e = f$."

The next remark that has to be made is that, even when the conductors, b and d , to be compared are endowed with sensible inductances (positive or negative), the problem may still, theoretically, be brought under the above head. Suppose, for example, that b , d represent nearly equal electromagnets. Their inductances may be compensated by the introduction (in series) of suitable equal condensers into these branches, so that b and d are reduced to b_1 and d_1 . If then we assume a to be a simple resistance ($a_2 = 0$), the solution is as before. Two objections may here be raised. First, on the theoretical side it has not been proved to be advantageous to assume $a_2 = 0$; and, secondly, the introduction of extraneous condensers,* even with interchange, into the branches to be accurately compared may be a complication unfavourable to success.

We will now resume the consideration of (19), supposing that

$$e = e_1 + ie_2 = r_1 - ir_2, \quad f = f_1 + if_2 = s_1 - is_2 \quad \dots \quad (28),$$

r_1, r_2, s_1, s_2 being given by (20), (21). Thus,

$$\psi_2/\Psi_1 = \frac{d-b}{16} \frac{s_1 + is_2}{br_1s_1} \dots \dots \dots (29),$$

and the question before us is how to make the modulus of the second fraction on the right a maximum by variation of a . In the denominator of this fraction r_1 and s_1 are real, and the modulus of b is $\sqrt{(b_1^2 + b_2^2)}$. For the numerator we have

$$\frac{1}{a} + \frac{1}{b} = \frac{1}{a_1 + ia_2} + \frac{1}{b_1 + ib_2} = \frac{2}{s_1 + is_2} = \frac{2(s_1 - is_2)}{s_1^2 + s_2^2},$$

so that

$$\frac{2s_1}{s_1^2 + s_2^2} = \frac{a_1}{a_1^2 + a_2^2} + \frac{b_1}{b_1^2 + b_2^2}.$$

Also from the definition of s

$$s_1^2 + s_2^2 = \frac{4(a_1^2 + a_2^2)(b_1^2 + b_2^2)}{(a_1 + b_1)^2 + (a_2 + b_2)^2};$$

so that

$$\frac{s_1^2}{s_1^2 + s_2^2} = \frac{(a_1^2 + a_2^2)(b_1^2 + b_2^2)}{(a_1 + b_1)^2 + (a_2 + b_2)^2} \left\{ \frac{a_1}{a_1^2 + a_2^2} + \frac{b_1}{b_1^2 + b_2^2} \right\}^2.$$

Thus

$$\text{Mod } \frac{br_1s_1}{s_1 + is_2} = \frac{(a_1 + b_1) \{a_1(b_1^2 + b_2^2) + b_1(a_1^2 + a_2^2)\}}{2\sqrt{(a_2^2 + a_2^2)} \cdot \sqrt{\{(a_1 + b_1)^2 + (a_2 + b_2)^2\}}} \dots (30),$$

and this is to be made a minimum by variation of a_1, a_2 .

* The use of condensers or electromagnets in the branches e and f stands, of course, upon a different footing.

We shall show presently that (30) can be reduced to zero; but for the moment we will so far limit the generality of a_1, a_2 as to suppose that $a_1 = xb_1, a_2 = xb_2, x$ being real and positive.

(30) then reduces to $\frac{1}{2} b_1^2(1+x)$; and by (29)

$$\text{Mod } \psi_2/\Psi_1 = \frac{\text{Mod } (d-b)}{8b_1^2(1+x)} \dots\dots\dots (31).$$

Accordingly, the maximum sensitiveness cannot be attained until x is reduced to zero, so that a_1, a_2 vanish. (31) may be regarded as a generalised form of (24), free from the limitation that $b_2 = 0$, provided a_2 be so taken that $a_2/b_2 = a_1/b_1$.

We will now suppose in (30) that a_1 and a_2 are both small, and in the first instance that b_1 is finite. We have

$$\frac{1}{2} b_1 \sqrt{(b_1^2 + b_2^2)} \frac{a_1}{\sqrt{(a_1^2 + a_2^2)}} + \frac{\frac{1}{2} b_1^2}{\sqrt{(b_1^2 + b_2^2)}} \sqrt{(a_1^2 + a_2^2)} \dots (32);$$

and this reduces ultimately to its first term, depending upon the ratio only of a_1 and a_2 . The expression vanishes if $a_1 : a_2$ be small enough, so that (30) can certainly be thus reduced to zero. It is remarkable that the expression for the sensitiveness should be capable of becoming infinite by suitable choice of a_2 . If we first suppose that a_2 is absolutely zero, and afterwards that a_1 diminishes without limit, the ultimate value of (32) is $\frac{1}{2} b_1 \sqrt{(b_1^2 + b_2^2)}$, in place of zero.

From the practical point of view, these conclusions from our equations are not particularly satisfactory. We began with certain proposals which, in ordinary cases, could be carried out; but in the end we are directed to apply them to an extreme and impossible state of things. We have found, however, in what direction we must tend in the search for sensitiveness; and useful information may be gathered from (32). In practice a_1 could not be reduced below a certain point. The question may then be asked, what is the best value of a_2 , when a_1 is given? From (32) we find at once that

$$a_1^2 + a_2^2 = \frac{a_1 (b_1^2 + b_2^2)}{b_1} \dots\dots\dots (33),$$

(32) then becoming

$$b_1 \sqrt{(a_1 b_1)} \dots\dots\dots (34).$$

In this case from (29)

$$\text{Mod } \psi_2/\Psi_1 = \frac{\text{Mod } (d-b)}{16b_1 \sqrt{(a_1 b_1)}} \dots\dots\dots (35),$$

independent of b_2 .

If we suppose in (32) that $a_2 = 0$, we have

$$\frac{1}{2} b_1 \sqrt{(b_1^2 + b_2^2)} + \frac{\frac{1}{2} b_1^2 a_1}{\sqrt{(b_2^2 + b_2^2)}} \dots\dots\dots (36).$$

To take a numerical example, let $b_2 = 0$; and suppose $a_1 = \frac{1}{10} b_1$. Then, according to (33), $a_2 = \pm \frac{3}{10} b_1$. Also by (20), (21),

$$\begin{aligned} e_1 &= \frac{1}{20} b_1, & e_2 &= \mp \frac{3}{20} b_1; \\ f_1 &= \frac{4}{15} b_1, & f_2 &= \mp \frac{6}{15} b_1. \end{aligned}$$

The corresponding minimum value of (32), equal to (34), is $b_1^2/\sqrt{10}$.

But with this value of a_1 the gain by allowing a_2 to be finite is not great. If $a_2 = 0$,

$$\begin{aligned} e_1 &= \frac{1}{20} b_1, & e_2 &= 0; \\ f_2 &= \frac{1}{6} b_1, & f_2 &= 0; \end{aligned}$$

and the value of (32), equal to (36), is $\frac{1}{30} b_1^2$.

We see from (36) that when $a_2 = 0$ there is little to be gained by further reduction of a_1 . But when a_2 is suitably chosen the gain may be worth having. Thus, in (34), if $a_1 = \frac{1}{100} b_1$, we have $\frac{1}{10} b_1^2$. Corresponding to this $a_2 = \pm \frac{1}{10} b_1$ nearly, and

$$\begin{aligned} e_1 &= \frac{1}{2} b_1, & e_2 &= \mp \frac{1}{20} b_1; \\ f_1 &= \frac{1}{25} b_1, & f_2 &= \mp \frac{1}{6} b_1. \end{aligned}$$

These are not unreasonable proportions, and we see that the use of a_2 may be advantageous, even when the subject of measurement is a mere resistance. It will be remarked too that, except as regards e_2, f_2 , the sign of a_2 is immaterial.

When the branches b, d consist of electromagnets, and still more when they consist of condensers, b_1 may be very small. If we suppose it to be zero, (30) becomes

$$\frac{a_1^2 b_2^2}{2\sqrt{(a_1^2 + a_2^2)} \cdot \sqrt{\{a_1^2 + (a_2 + b_2)^2\}}} \dots\dots\dots (37).$$

Corresponding to this from (20), (21),

$$e_1 = \frac{1}{2} a_1, \quad e_2 = -\frac{1}{2} (a_2 + b_2) \dots\dots\dots (38),$$

$$f_1 = \frac{2a_1 b_2^2}{a_1^2 + (a_2 + b_2)^2}, \quad f_2 = -\frac{2a_1^2 b_2 + 2a_2 b_2 (a_2 + b_2)}{a_1^2 + (a_2 + b_2)^2} \dots (39).$$

From (37) we see that the increase of a_2 is favourable, especially if the sign be the same as of b_2 . Even if $a_2 = 0$, (37) now assuming the form

$$\frac{a_1 b_2^2}{2\sqrt{(a_1^2 + b_2^2)}} \dots\dots\dots (40)$$

can be reduced to zero by taking a_1 small enough. But of course (37) ceases to be applicable unless b_1 be small relatively to a_1 . In correspondence with (40),

$$e_1 = \frac{1}{2} a_1, \quad e_2 = -\frac{1}{2} b_2 \dots\dots\dots (41);$$

$$f_1 = \frac{2 a_1 b_2^2}{a_1^2 + b_2^2}, \quad f_2 = -\frac{2 a_1^2 b_2}{a_1^2 + b_2^2} \dots\dots\dots (42).$$

As an example of (37), suppose

$$a_1 = \frac{1}{4} b_2, \quad a_2 = 4 b_2.$$

Then $(37) = \frac{b_2^2}{640}$ nearly.

Also approximately

$$e_1 = \frac{1}{8} b_2, \quad e_2 = -\frac{5}{2} b_2, \quad f_1 = \frac{1}{80} b_2, \quad f_2 = -\frac{8}{5} b_2.$$

If b_2 represent the stiffness of a condenser, f_2 must be a positive inductance, and its magnitude, relatively to f_1 , would probably constitute a difficulty.

As an example, with a_2 equal to zero, take

$$a_1 = \frac{1}{10} b_2, \quad a_2 = 0.$$

Then $(37) = (40) = \frac{1}{20} b_2^2$ nearly,

and

$$e_1 = \frac{1}{20} b_2, \quad e_2 = -\frac{1}{2} b_2, \quad f_1 = \frac{1}{5} b_2, \quad f_2 = -\frac{1}{80} b_2.$$

So far as the general theory is concerned, it is a matter of indifference whether the indicating instrument be in the branch e , or in f . The latter corresponds to the connections in De Sauty's method of testing condensers by means of the galvanometer. In practice, more space would probably be available for the coils of a transmitting instrument than of the receiving instrument, at least, if the latter be a telephone; and this would tell in favour of choosing that branch for the transmitter which should have the larger time constant (L/R).

To get an idea of the relative capacities, resistances, and inductances involved, we must assume a particular pitch. A frequency suitable for telephonic experiments is 1000 per second, for which $p = 2000\pi$. Thus, if the value of a_2 for a condenser of capacity C , and for an inductance L , and that of a_1 for a resistance R , are all numerically equal,

$$R = 2000\pi L = \frac{1}{2000\pi C}.$$

If R be 1 ohm, equal to 10^9 C.G.S., the corresponding capacity is 1.6×10^{-13} C.G.S., equal to 160 microfarads, and the corresponding

inductance is 1.6×10^5 C.G.S. Again, if C be one microfarad, equal to 10^{-18} C.G.S., R is 160 ohms, and L is 2.5×10^7 cm.

In the preceding calculations e and f are supposed to be adjusted to the values most favourable to the effect in the receiving instrument. A question, which arises quite as often in practice, is how to make the best of given instruments. The full answer is necessarily somewhat complicated; for there could be no objection to the insertion of a condenser for example, if the sensitiveness could be improved thereby. In what follows, however, the transmitting and receiving branches will be supposed to be fully given, so that e and f are known complex quantities; and the only question to be considered is as to the most suitable value of a , assumed to be equal to c .

For this purpose the modulus of the second fraction on the right in (19) is to be a maximum, or that of

$$(a+b+2e) \left(\frac{1}{a} + \frac{1}{b} + \frac{2}{f} \right) \dots\dots\dots (43)$$

is to be a minimum, by variation of a . The problem thus arising of determining the minimum modulus of a function of a complex quantity may be treated generally.

Let

$$F(z) = F(x+iy) = \phi(x, y) + i\psi(x, y),$$

and let it be required to find when the modulus² of $F(z)$, viz., $\phi^2 + \psi^2$, is a minimum by variation of x, y . We have

$$\phi \frac{d\phi}{dx} + \psi \frac{d\psi}{dx} = 0, \quad \phi \frac{d\phi}{dy} + \psi \frac{d\psi}{dy} = 0 \dots\dots (44).$$

And in general

$$\frac{d\phi}{dx} = \frac{d\psi}{dy}, \quad \frac{d\phi}{dy} = -\frac{d\psi}{dx} \dots\dots (45).$$

In order that (44), (45) may both obtain, we must have either $\phi^2 + \psi^2 = 0$, or else

$$\frac{d\phi}{dx} = 0, \quad \frac{d\phi}{dy} = 0, \quad \frac{d\psi}{dx} = 0, \quad \frac{d\psi}{dy} = 0.$$

The latter conditions are equivalent to

$$F'(z) = 0 \dots\dots\dots (46).$$

For example, let

$$F(z) = (z+\alpha) \left(\frac{1}{z} + \beta \right) \dots\dots\dots (47),$$

where α, β are complex constants.

The application of (46) gives

$$z^2 = \alpha/\beta \dots\dots\dots (48),$$

and $F(z) = \{1 + \sqrt{(\alpha/\beta)}\}^2 \dots\dots\dots (49).$

We see then that the modulus of (43) will be a minimum, when

$$a^2 = \frac{b+2e}{2/f+1/b} \dots\dots\dots (50),$$

and in taking the square root the ambiguity must be so determined as to make the real part of a positive.

Equation (50) coincides with that obtained by Mr. Heaviside for the case where all the quantities are real.

II. "On the Influence of Pressure on the Spectra of Flames."

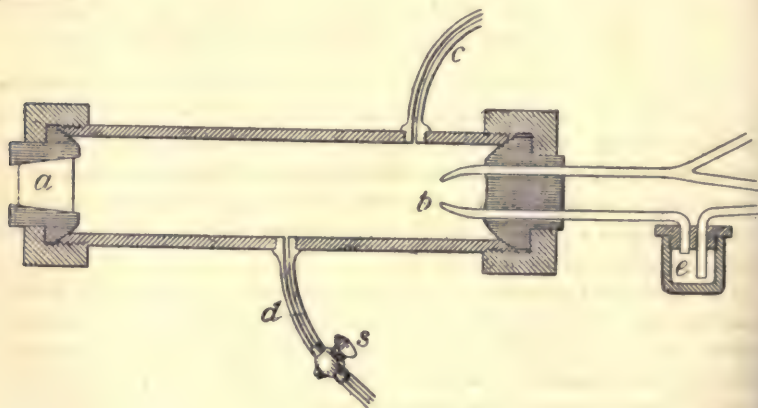
By G. D. LIVEING, M.A., F.R.S., Professor of Chemistry,
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University of Cambridge. Received January 22, 1891.

We have already described ('Phil. Trans.,' A, 1888) the remarkable spectrum of the oxy-hydrogen flame burning at the ordinary atmospheric pressure. Recently we have examined the spectrum of the same flame at various pressures: hydrogen burning in excess of oxygen up to a pressure of 40 atmospheres, and oxygen in excess of hydrogen up to a pressure of 25 atmospheres, also that of the mixed gases burning in carbonic acid gas.

The apparatus employed was an adaptation of one of the tubes used in our experiments on the absorption spectra of compressed gases ('Phil. Mag.,' September, 1888, and 'Roy. Soc. Proc.,' vol. 46, p. 222). It consisted of a steel cylinder, about 50 mm. in internal diameter and 225 mm. long, fitted at one end with a quartz stopper, a , in the annexed figure, and with a jet, b , for burning the gas, adapted by a properly fitting union joint to the opposite end. There were two tubes, c and d , connected to the cylinder at the sides, of which one, c , served for the introduction of gas, while the other, d , was fitted with a stopcock and was used to draw off the water formed, or to reduce the pressure of the gas in the cylinder if that was desired. The flame was observed, nearly end on, through the quartz stopper. The whole apparatus was kept cool by a stream of cold water running on to a sponge cloth wrapped round the cylinder. In the course of the tube conveying gas to the jet b was interposed a small cylinder, e , in which sodium was placed, and by heating this, the gas entering could be charged with sodium vapour.

The gases were supplied from steel cylinders into which they had been compressed, and the pressure was registered by a gauge attached to the tube by which the gas entered the experimental cylinder. Commercial compressed gases were used, containing a sensible percentage of air.

When hydrogen was the gas forming the burning jet, it was lighted at the end of the tube *b* before introducing it into the experimental cylinder. When it was desired to have a jet of oxygen burning in hydrogen, this could be managed by introducing oxygen through the second tube and increasing the supply of hydrogen until the flame passed over to the oxygen jet. The same result was sometimes attained by first filling the experimental cylinder by a gentle stream of hydrogen through the side tube *c* before the end with the tube *b* was screwed on; the hydrogen as it issued was then lighted, and the jet, with a gentle stream of oxygen issuing, inserted and screwed down. The stopcock *s* was kept open until this was done, and then by closing *s*, and admitting more gas from the reservoirs, the pressure in the experimental cylinder could be increased at pleasure.



Hydrogen Burning in Oxygen.

The first observations were made with a jet of hydrogen burning in oxygen. As the pressure rose, the luminosity of the flame increased, as long ago described by Frankland ('Experimental Researches,' p. 905). The colour of the flame, viewed end on, was yellow, as if it contained sodium; but, on examining it with a spectroscope, it was found to give a continuous spectrum intersected by many shaded bands, and the D lines of sodium were only faintly present. The shaded bands were faint at a pressure of 5 atmospheres, but at pressures of 20 atmospheres and upwards they came out strongly. They were evidently the absorption bands of NO_2 .

derived from the residue of atmospheric air mixed with the condensed gases. We took a photograph of them, and on comparing this with a photograph of the NO_2 bands, we found the two to be identical. Except for the bands, and the bright lines of sodium, the spectrum appeared to be continuous, and to extend from about λ 6200 to λ 4150, with the brightest part about λ 5150. It increased in brilliance as the pressure increased, as well as in extent, being visible at 3 atmospheres pressure from about λ 6720 to λ 4040. The greater distinctness of the NO_2 bands at the higher pressures was due both to the greater brightness of the continuous spectrum and to the greater quantity of NO_2 formed. A large quantity of water accumulated in the experimental tube, and when this was drawn off by the stopcock *s*, it effervesced with escape of NO , and was found to be strongly acid. A specimen titrated was found to contain very nearly 3 per cent. of nitric acid. The observations were continued up to a pressure of 40 atmospheres. There was no indication that the continuous spectrum had any connexion with the line spectrum of hydrogen. There was no increase of brilliance in the neighbourhood of the C, F, or G lines of hydrogen. The characters of the spectrum were, however, better seen in the absence of NO_2 , and will be described in the next section.

Oxygen Burning in Hydrogen.

In this case the colour of the flame was very different from that of hydrogen burning in oxygen. Instead of being yellow, it appeared, to the unaided eye, to have a lavender hue. In the spectroscope it showed a perfectly continuous spectrum, brightest in the green, about the region of the Fraunhofer line *b*, and very gradually fading away on either side. On the red side it could be just traced up to about λ 6150, and on the violet side to about λ 4285, at ordinary pressures. The sodium lines were absent. With increase of pressure it increased very much in brightness, and at 8 atmospheres pressure it could be traced as low as λ 6630 and as high as λ 3990.

The dispersion used was that of a direct-vision spectroscope (such as was described by us, 'Roy. Soc. Proc.' vol. 41, p. 449), equivalent to three prisms of white flint glass, but the collimator and telescope very short, so as to obtain plenty of light. With less dispersion, perhaps, the continuous spectrum might have been traced further. Photographs, however, showed that it scarcely extended into the ultra-violet. There was no indication that this spectrum was due to an expansion of the lines of either the first, or second, spectrum of hydrogen. It is true that the maximum brightness (which could not be determined with any great accuracy) was not very far from F, but no indication of any second maximum in the neighbourhood of either

C or G, or anywhere else, could be detected. The pressure was carried up to 12 atmospheres, and at this pressure the visible spectrum was brilliant, but, in the ultra-violet, photographs showed that the spectrum consisted only of what we have called the "water-spectrum," very strong and sharp. The lines of this spectrum showed no signs of expansion even at a pressure of 12 atmospheres, and, though much more intense than at ordinary pressures, remained clearly defined.

Observations were continued with the eye up to 25 atmospheres pressure, but no trace of emission, or absorption, corresponding to either spectrum of hydrogen could be detected, and it is doubtful if either spectrum can be produced in such a flame. Since the formation of steam from its component gases is attended with a diminution of volume, increased pressure will increase the stability of the compound, and the flame will contain a larger proportion of steam, as well as have a higher temperature, than at ordinary pressures.

The water formed when the flame was a jet of oxygen burning in hydrogen was found to be alkaline, and to contain ammonia. But the proportion of ammonia was much less than the proportion of nitric acid formed when the jet was hydrogen burning in oxygen; a specimen titrated contained 0.004 per cent. of ammonia.

Effects of Pressure on the Sodium Spectrum.

In order to see what effect would be produced by increased pressure on the spectrum of other substances in the flame, we charged the hydrogen with sodium vapour by making it pass, before entering the experimental cylinder, through a small iron cylinder, *c* in the figure, containing metallic sodium, heated by a lamp. As the D lines of sodium are very easily expanded and self-reversed in a flame at ordinary pressure, some care was needed to discriminate the effects which were really to be ascribed to pressure. The gas was easily charged with sodium vapour, and when burning in oxygen, not only the D lines, but the citron and green pairs, and sometimes the blue pair (λ 467), and the orange pair (λ 616), were well seen; but we could not find that they were expanded by increase of pressure. A sudden change of pressure generally produced an expansion, but it did not last; the lines fined down again when the pressure was steady, whether that pressure was high or low. These experiments were continued up to a pressure of 40 atmospheres without any definite effect on the width of the lines which could be ascribed to the pressure.

It may be said that at the higher pressure the evaporation of the sodium would be slower, and so the proportion of sodium vapour to hydrogen be diminished; also, when the lines are diffuse at the

edges to begin with, it is extremely difficult to judge whether there is any expansion. At all events, we may say that there is no expansion produced by pressure at all comparable with that produced in a flame at ordinary pressure by increasing the quantity of sodium in the flame. We noticed, however, that the presence of sodium, which produces a feeble continuous spectrum in a flame at an ordinary pressure, seemed to increase the continuous spectrum of the flame under pressure, especially in the orange and green.

Oxy-hydrogen Jet in Carbonic Acid Gas.

For this experiment a two-branched tube (the upper one in the figure) was used. The jet of mixed oxygen and hydrogen was first lighted and introduced into the experimental cylinder while the latter was full of air and the stopcock *s* open. The air was then replaced by CO_2 entering by the tube *c*. The effect of this was at once to brighten the flame and change its colour from yellow to blue. Seen in the spectroscope, the change consisted in an increase of continuous spectrum, especially towards the more refrangible end. When the stopcock *s* was closed so that the pressure rose in the experimental cylinder, the flame increased in brightness, but there was no other change in the spectrum. It remained continuous with no bright or dark lines, or bands, except the D lines of sodium. It resembled an ordinary flame of CO. The jet would not burn in CO_2 unless there was some excess of oxygen, and even with an excess of oxygen we could not get it to continue to burn in CO_2 at a pressure higher than 2 atmospheres.

Ethylene in Oxygen.

A jet of ethylene burning in oxygen gave, when the flame was small, the usual candle-flame spectrum, together with a band in the indigo (λ 431) shading towards the violet; but as the pressure was increased the continuous spectrum brightened and completely overpowered the bands, and at the same time the absorption spectrum of NO_2 appeared. We carried the pressure up to 33 atmospheres, and at that pressure the flame seemed to give nothing but a continuous spectrum, intersected by the absorption bands of NO_2 . In our tube, the flame was viewed almost directly end on, and it is possible that if we had seen the flame sideways, we might have detected the hydrocarbon flame spectrum near the nozzle. At the high pressure much soot separated. We tried burning a mixture of ethylene and oxygen. The mixed jet burnt well in air and, when the supply of oxygen was sufficient, gave the hydrocarbon flame spectrum. In the experimental tube in oxygen, the jet burnt well at the atmospheric pressure,

but we failed to get it to continue burning when the pressure was increased. The shaded band, commencing with a sharply-defined edge about λ 431, seems to be independent of the pressure, and has been before observed in a gas flame (Huggins, 'Roy. Soc. Proc.,' vol. 30, p. 580). In fact, the only effect of pressure in this, as in the former cases, seemed to be the increase of the continuous spectrum.

Cyanogen and Oxygen.

As we could not obtain cyanogen at such pressures as we had used in the case of the other gases, we were obliged to content ourselves with exploding mixtures of cyanogen and oxygen in an iron bottle, fitted with a quartz stopper like that of the experimental tube above described. The bottle, having been exhausted by an air-pump, was filled with the mixture of gases, and exploded by an electric spark. With less than 3 vols. of oxygen to 1 vol. of cyanogen, there was always a considerable deposit of carbon, which covered the quartz and impeded vision; but, with 3 vols. of oxygen to 1 of cyanogen, the carbon was all burnt. Notwithstanding the brilliant banded spectrum of a flame of cyanogen in oxygen at ordinary pressure, nothing but a continuous spectrum could be seen in the flash of the exploded gases, except the ubiquitous D lines of sodium. The continuous spectrum was bright. Photographs showed a continuous spectrum with lines of iron, calcium, potassium, and sodium, but no cyanogen or carbon bands, or carbon lines. When a little hydrogen was added to the mixture of gases, no trace of the hydrogen red or green line could be detected in the spectrum of the exploding gas.

In every case, the prominent feature of the light emitted by flames at high pressure appears to be a strong continuous spectrum. There is not the slightest indication that this continuous spectrum is produced by the widening of the lines, or obliteration of the inequalities, of the discontinuous spectra produced by the same gases at lower pressures. On the contrary, it seems to be developed independently. This is, on the whole, quite in accordance with what would be expected, considering that under pressure the molecules of the gases have much less freedom, encounters amongst them are much more frequent, and they have much less chance of vibrating independently, and of taking up exclusively, or chiefly, the fundamental rates of vibration which are natural to them when free. Their condition, during a large part of any given time, approximates to that of the molecules of a liquid, and their spectra approximate to that of a liquid to at least a like extent. On the other hand, the higher temperature which, in many flames, attends an increased pressure ought to give some intensity to the special radiation which the molecules emit during their time of free motion; and this we have

noticed to occur in the principal sections of the discontinuous spectrum of the oxy-hydrogen flame. Whether the continuous spectrum is due to the mutual action of the molecules of the compressed gases may perhaps be best determined by some photometric measures of the rate at which the brilliance increases with the pressure. Frankland ('Exp. Researches,' pp. 892 *et seq.*) has made some such measures, but not sufficient to solve the question. We have made an attempt to measure, not the total intensity of the light, but that of rays of definite refrangibility.

Photometry of Oxy-Hydrogen Flame under Pressure.

The apparatus used for these measures was a spectro-photometer of the pattern employed by Crova ('Annales de Chimie,' ser. 5, vol. 29, p. 556). In this, the rays of one of the sources of light to be compared are passed through two Nicol's prisms, and then reflected into one half of the slit of the spectroscope, while the light from the other source passes directly into the other half of the slit. By turning one of the Nicol prisms, the light from the first source can be reduced at pleasure, and any small section of the spectrum can be separately observed by cutting off the rest by means of a shutter in the eye-piece. We found it by no means easy to get good concordant observations. A much larger vessel was used than for the earlier experiments, one which contained several litres, and so we may presume a more uniform pressure was maintained within it. The results of the best series of observations on the photometric intensity of the jet of oxygen burning in hydrogen are given in the following table. The comparison light was a petroleum lamp.

1.	2.	3.	4.
15 lbs.	3°	274	$30 \times 3^2 = 270$
35	7	1485	$30 \times 7^2 = 1470$
55	11	3641	$30 \times 11^2 = 3630$
75	14	5853	$26 \times 15^2 = 5850$
95	19	10600	$29 \times 19^2 = 10469$

The first column gives the pressure of the gas, the second the mean of four to six observations of the angular deviation of the Nicol's prisms from the position of complete extinction, for each pressure. The third column gives the squares of the sines of the angles in the second column multiplied by 100,000.

It will be seen from the last column that the numbers in the third column, which should be proportional to the photometric intensities

at the respective pressures, are approximately proportional to the squares of the pressures.

This may be taken to indicate that the brightness of the continuous spectrum depends mainly on the mutual action of the molecules of gas.

A series of similar observations on hydrogen burning in oxygen gave somewhat different results, tabulated below :—

1.	2.	3.
15 lbs.	6°	1093
35	13	5060
55	18	9549
75	22	14033
95	28	17861

The flame was brighter than that of oxygen burning in hydrogen at ordinary pressure, but the rate of increase with increased pressure was not so rapid as in the former case. It seems as if the continuous spectrum were made up of two parts, one varying as the square of the pressure, and another according to some other law. The flame is evidently not the same in the two cases. The products of combustion derived from the small quantity of air are different, and also the hydrogen jet always showed the presence of sodium, sometimes calcium. The appearance of the flame was also different; the hydrogen jet being faintly visible and yellowish in the elongated part, whereas the light from the oxygen jet was concentrated near the base, the point being invisible. The measures of which the means are tabulated above were also less concordant than the corresponding measures for the oxygen jet. We were unable to carry our measures beyond a pressure of 95 lbs., because at higher pressures a cloud was formed in the apparatus which prevented our seeing the flame directly. We hope to prosecute these measures with flames of other gases, and, if possible, at higher pressures.

The conclusions to which our experiments have led seem inconsistent with those which have been drawn from Plücker and Hittorf's well-known observations on the widening of the hydrogen lines in vacuous tubes with a residue of hydrogen when that residue increases. That the widening of the lines in a Plücker's tube results from increasing the density of the residue of hydrogen in the tube cannot be gainsaid, but we are wholly ignorant of the mechanism by which the gas is lighted up by the electric discharge. It is sometimes assumed, but without any sufficient reason, that the energy of the electric current is first converted into heat, and then in turn into

radiation; but the electric energy may equally well be directly converted into the motion of radiation. As a fact, we have never yet been able to obtain either the emission or the absorption spectrum of hydrogen without the aid of an electric current, so that, in reasoning on this spectrum, we are much more in a region of speculation than when treating of flames. Whether the hydrogen lines, bright or dark, in the solar spectrum are produced directly by the high temperature of the sun, may even be called in question. And though we may admit that the density of the hydrogen in the sun's atmosphere, outside the photosphere, is but slight, it does not follow that the total pressure of all the gases forming that atmosphere is so very small as Messrs. Frankland and Lockyer ('Roy. Soc. Proc.,' vol. 17, p. 288) have, from the width of the lines, concluded it to be. After all, it is not so easy to connect the temperature, even of a flame, with its radiation, for it is only when the condition of a gas is steady that we can assume that there is a definite relation between the motion of agitation, on which temperature depends, and the vibratory motions, on which radiation depends. In speculating on such questions, chemical, as well as electrical, changes must not be lost sight of, although the latter may be more directly concerned in radiation.

Experiments which we have commenced upon the arc in an atmosphere of compressed gas tend to the same conclusion. It does not appear that the metallic lines in the arc are sensibly affected by a steady pressure up to 15 atmospheres. The details of these observations, which are complicated by the variation of resistance with change of pressure, we defer until the experiments are finished.

III. "On the Focometry of Lenses and Lens-Combinations, and on a new Focometer." By SILVANUS P. THOMPSON, D.Sc., B.A., Professor of Physics in the City and Guilds Technical College, Finsbury. Communicated by Professor G. CAREY FOSTER, B.A., B.Sc., F.R.S. Received February 4, 1891.

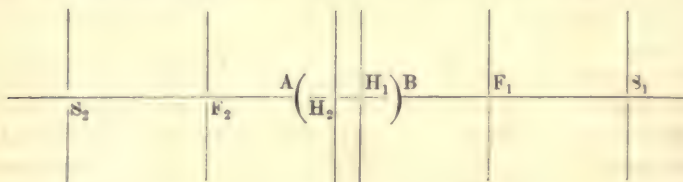
(Abstract.)

Few of the accepted methods of focometry take into account the distance between the two principal points (or Gauss points) of a lens, or afford the means of measuring this distance, as well as the true focal length, and some of them are open to the objection that they necessitate troublesome double adjustments. Of these methods the author gives a brief categorical review.

He has devised a method in which there are no double adjustments, no measurements of the size of optical images, no assumptions as to

the approximate positions or distance apart of the two principal points, but in which both the true focal length and the width between the principal points are determined by direct measurements of lengths.

The principle of the method is as follows:—Beyond the principal focal points on each side of the lens, at distances equal to the true focal length, are two points which are conjugate to one another and symmetrically situated at twice the true focal length from the two principal points. These may be called the symmetric points: and the planes drawn through them orthogonally to the principal axis may be called the symmetric planes. They are planes of unit magnification, and possess the known geometric property that the ordinate in one of these planes of the point of intersection of any incident ray is equal in magnitude, but opposite in sign, to the ordinate in the other plane of the point of intersection of the emergent ray. Let AB be the lens or combination of lenses, F_1F_2 the principal foci, H_1H_2 the principal points, S_1S_2 the symmetric points. Then the true focal length is $F_2H_2 = F_1H_1 = F_1S_1 = F_2S_2$.



Suppose a parallel beam to be sent from left to right through AB ; an image will be formed at F_1 . Let the light then be sent from right to left forming an image at F_2 . Suitable transparent micrometers are placed to receive these images and to ascertain their precise position in space. A graduated bench is provided upon which the lens and the micrometers are placed so as to read off the distances between these points. A gearing is provided, namely, a right- and left-handed screw, by means of which, when the two micrometers have been placed at F_1 and F_2 and clamped to the screw, they can be moved by the experimenter at exactly equal rates outwards, so that when one arrives at S_1 the other arrives at S_2 . This is known by observing in one micrometer the exact image of the other of equal size. The distance through which the micrometers have each been displaced is equal to the true focal length; and the distance H_1H_2 between the two principal points is found by reckoning backwards from F_1 and F_2 distances equal to the focal length so found. The positions of the two principal points can then be marked upon the outside of the tube of the objective.

These principles are embodied in an instrument described in the

paper, and called a *focometer*. It has been constructed to the author's designs by Messrs. Nalder Brothers, to whom sundry of the mechanical details are due.

The paper also describes the results obtained with the focometer upon various lenses, some of them being microscope objectives, others camera lenses. The author finds in several of these lenses that the principal planes are crossed: the distance between the symmetric points being less than four times the focal length. In some other lenses which are achromatic in respect of bringing all rays to a common principal focus, the positions of the principal planes are different for rays of different colours. In one lens, a microscope objective by Reichert, the principal planes are not only crossed but are actually at a greater distance apart than the two principal foci. The paper is accompanied by a sheet of full-size drawings showing the construction of the instrument and its details.

IV. "The Numerical Registration of Colour. Preliminary Note."

By Captain W. DE W. ABNEY, C.B., R.E., D.C.L., F.R.S.

Received February 6, 1891.

The Committee of the Royal Society on Colour Vision having put into my hands the determination of the colour of certain signal glasses, a memorandum was drawn up on the method of the numerical registration of colours and submitted to them. They considered that it should be submitted to the Royal Society, and having slightly modified it, it is presented as a preliminary note of a part of a paper which will be subsequently submitted by General Festing and myself as Part III of "Colour Photometry."

It must be premised that a colour is determined when its hue, its purity, and its luminosity are known, the last constant being its comparison with the white light before its passage through a transparent coloured body, or with white light reflected from a white surface if it be an opaque coloured body such as a pigment.

There has hitherto been a certain amount of difficulty on the part of normal-eyed persons in stating the exact hue of compound colours in terms of any standard; in fact, I believe, except by the method given in the Second Part of "Colour Photometry" ('Phil. Trans.,' A, 1888), there has been no exact means indicated of reproducing a colour from measurements made. The method which will be described can take the place of the previous plan for certain purposes, more particularly when it is the impression on the eye which has to be considered. Any colour can be reproduced from the registration numbers with the greatest exactness.

To persons who are totally colour-blind to one sensation, viz., the

green or the red, the matching of a compound colour with a simple one in the spectrum should possess no difficulties. Taking the trichromatic theory of three sensations for the normal-eyed person, it is evident that only the following classes of sensations are possible in the normal-eyed, the green colour-blind, and the red colour-blind :—

Normal eye.	Green colour-blind.	Red colour-blind.
Red	Red	—
Green	—	Green.
Violet	Violet.....	Violet.
Mixtures of red and green	—	—
Mixtures of red and violet.....	Mixtures of red and violet.	—
Mixtures of green and violet	—	Mixtures of green and violet.
Mixtures of red, green, and violet	—	—

If we take as a type of colour-blindness the green colour-blind person, we see that every colour in the spectrum must be either red, violet, or these colours mixed with more or less white light, since these two sensations when excited in certain proportions give the sensation of white. At one place, which is commonly called the neutral point, these proportions are such that there is the impression of white light ; it follows that, between this neutral point and each end of the spectrum, the rays are mixtures of violet and white or red and white, the dilution of the colours varying from no white to all white. As every compound colour must be a mixture of the same two colours in certain proportions, it follows that the green colour-blind person can match every compound colour with some one ray of the spectrum, and that every colour must to him be either red or violet, diluted with different proportions of white light.

In the same way, a person who is colour-blind to the red can match any colour with a single spectrum colour, and he will see it as green or violet diluted with more or less white light. This can be readily understood, but it is not quite so plain how any colour sensation felt by the normal eye can be referred to the spectrum.

The following is an outline of the reasoning which leads up to the method of registration employed :—

If we take three rays in the spectrum—one in the red between C and the red lithium line, which we will call *R*, another in the green between F and *b*, which we will call *G*, and a third in the violet near G, but on the H side of it, and which we may call *V*—then, by varying their intensities (which is equivalent to varying the luminosities)

and mixing them, we can give the same impression to the eye that any compound colour gives, and that of any intermediate simple spectrum colour but very slightly diluted with white light. With these same three colours, but in different proportions, we can also give the impression of white light to the eye. The intermediate spectrum colours between the green and the violet rays selected, when slightly diluted, are imitated by mixing these rays together in different proportions, and similarly those lying between the red and the green by mixing together these rays in different proportions—and there is some ray present in the spectrum which, when very slightly diluted with white light, has the same colorific effect on the eye as the mixtures of the pairs V and G and G and R in any proportions whatever.

Let the luminosities of the rays R , G , and V , which give the impression of white light, be a , b , and c units respectively, and p , q , and r those which give that of the colour which has to be registered and reproduced. We then get the following equations—where W is white, w its luminosity, Z the colour, and z its luminosity—

$$aR + bG + cV = wW \dots\dots\dots (i);$$

$$pR + qG + rV = zZ \dots\dots\dots (ii).$$

Then evidently—

$$(a + b + c) = w, \quad \text{and} \quad (p + q + r) = z.$$

Let $p = \alpha a$, $q = \beta b$, $r = \gamma c$.

Then we may write (ii) as—

$$\alpha aR + \beta bG + \gamma cV = zZ \dots\dots\dots (iii).$$

Now, either α , β , or γ must be smaller than the other two. As an example, if α be the smallest, we multiply (i) by α , when we get—

$$\alpha aR + \alpha bG + \alpha cV = \alpha wW \dots\dots\dots (iv).$$

Subtracting (iv) from (iii), we get—

$$(\beta - \alpha) bG + (\gamma - \alpha) cV = zZ - \alpha wW.$$

Now, it has already been stated that between V and G there is some ray which gives the same sensation of colour, mixed with a very small quantity of white light, as the above mixture of V and G —let us call it X and its luminosity x [x being evidently equal to $(\beta - \alpha)b + (\gamma - \alpha)c$], and μ the luminosity of the small quantity of white added.

We then get $zZ = xX + (\mu + x)W$.

Here we have the colour Z in terms of a single ray, and of white light.

This same holds good when in (ii) γ is smaller than α and β ; but it does not do so should it happen that β is the smallest, for there is no part of the spectrum which contains simple colours giving the same sensation to the eye as mixtures of red and blue. There is, however, a very simple way in which the registration of such a colour (which it must be remarked must be of a purple tone) can be effected. It can be fixed by its complementary. To do this we must add to (ii) a certain amount of R and V , which will make the whole white. Thus, suppose in (iii) α to be larger than γ and γ than β , then we must add $\phi bG + \theta cV$, and we have—

$$\alpha aR + (\beta + \phi) bG + (\gamma + \theta) cV = nW = Z + \phi bG + \theta cV;$$

but $(\beta + \phi)$ and $(\gamma + \theta)$ each equal α ; $\therefore n = \alpha w$;

$$\therefore Z + \phi bG + \theta cV = \alpha wW.$$

Now, between V and G in the spectrum there is some single colour which gives the sensation of the mixture of G and V . Let it be X' with luminosity x' , together with white, whose luminosity is μ' , which equals $(\phi b + \theta c)$.

$$\therefore Z + x_1 X' + \mu' W = \alpha w W;$$

$$Z = (\alpha w - \mu') W - x' X';$$

which again is the colour expressed in terms of white light less the complementary colour. We have thus arrived at the very simple deduction that the hue and luminosity of any colour, however compounded, may be registered by a reference to white light and a single ray of the spectrum.

In practice this dominant ray is very easy to find. Suppose we wish to determine numerically the colour of a signal-green glass in the electric light; we should proceed as follows:—

The colour-patch apparatus described in the Appendix to the Bakerian Lecture "On Colour Photometry" ('Phil. Trans.,' 1886, Abney and Festing) is employed, and the coloured glass is placed between the silvered mirror, which reflects the beam already reflected from the first surface of the first prism of the spectrum apparatus, and the screen, and a square image of that surface of the prism, showing the tint of the glass, is formed on the screen by means of the lens. Touching this image is a square patch of white light, formed by the re-combination of the spectrum by means of another lens. An opaque slide, containing an adjustable slit, is moved across the spectrum in the manner described in the paper referred to, until

the colour of this last patch is approximately the same hue as that of the glass.

In the path of the reflected beam, but between the prism and the silvered mirror, is inserted a piece of plain glass, which can be made to reflect part of the white beam into the monochromatic patch of light, a square patch of this white light being formed by means of a third lens. We thus have monochromatic light mixed with white light. The requisite intensity of the added white light can be adjusted by means of rotating sectors which open and close at will during rotation, and the integrated luminosity of the mixed beams can be altered by this, together with the adjustable slit in the slide. The slit may probably have to be moved in the spectrum to make the hue of these mixed lights the same as that of the glass, but by trial the position of the ray, whose colour, when diluted with white light, makes the match, is readily found. The position of the slit in the spectrum is noted, as also the aperture of the sectors. The relative luminosities of the beam reflected from the plain glass mirror and of the coloured ray are next measured by placing a rod in the path of the two beams, and equalising by the sectors the luminosity of the shadows which are illuminated, the one by the spectral ray, and the other by the white light. When the sector aperture is noted the registration is complete, as far as hue is concerned, but the luminosity of the ray transmitted through the glass should be compared with that of the unabsorbed reflected beam, and then the total luminosity is doubly recorded.

Should the colour of a pigment be in question, the ray reflected from the silvered mirror is made to fall on the pigmented surface, and the same procedure adopted.

Should a purple glass (say) have to be registered, we proceed in a slightly different manner, the patch of colour light passing through the purple glass is superposed over the spectrum patch, and the slit in the slide is moved till a ray is found which will make white light when superposed on the colour of the glass. The luminosities of this white light, of the reflected beam, and of the spectral colour, are compared *inter se*, and there are then sufficient data on which to make numerical registration. In the paper which will be submitted to the Royal Society, and of part of which this is a preliminary note, the details of registration will be entered into more fully.

The signal glasses having to be used at night with oil or gas, their hue must be registered in these lights. As the spectrum colours are always the same, it is convenient to use the electric light spectrum, and the only alteration in the apparatus is to use two gas lights to illuminate two square apertures, in front of one of which the glass, whose colour has to be measured, is placed. The images of these apertures are thrown on the screen, the coloured image touching the

square image of the spectral colour patch, and the second image over the latter. The same determinations are gone through as those just determined.

The following are the determinations of some of the coloured glasses submitted to the Committee, recorded in this manner:—

Glass.	Electric light.			Gas light.		
	Domi- nant wave- length.	Per- centage of white light in colour.	Lumi- nosity, naked light = 100.	Domi- nant wave- length.	Per- centage of white light in colour.	Lumi- nosity, naked light = 100.
Great Western ruby glass	6250	7	10·4	6275	0	13·1
L.B.S.C.	6200	0	10·4	6200	12	13·0
Great Northern	6250	0	9·0	6275	0	10·0
Great Western signal green.....	4925	46	21·8	5070	50	18·1
L.B.S.C.	4925	38	16·2	5050	34	12·5
Great Northern	5100	61	19·2	5170	62	19·4
Great Eastern	5000	54	15·0	5120	40	15·0
Saxby and Farmer's, as ordinarily sup- plied where no special glass or- dered	4925	24	7·6	5050	22	6·9
Bottle green glass (District Railway)	5500	32	9·1	5320	50	10·6
Cobalt blue	4675	38	4·4	4650	59	3·3

The following are determinations of some coloured pigments in the electric light:—

Colours.	Dominant wave-length.	Percentage of white light.	Percentage of luminosity (white paper = 100).
Vermilion	6100	2·5	14·8
Emerald green	5220	59·0	22·7
F ultramarine	4720	61·0	4·4
Brown paper	5940	50·0	25·0
„ (greyer)	5670	67·0	19·5
Orange	5915	4·0	62·5
Chrome yellow	5835	26·0	77·7
Blue-green	5005	42·5	14·8
Eosin dye ('Sporting Times')	6400	72·0	44·7
Cobalt	4820	55·5	14·5

The determination of the colours in Maxwell's disks by this plan enables them to be used much more effectively than if they are simply indeterminate colours. For since the sum of the luminosities of any colours is equal to the luminosity of the same colours integrated, it follows that when using the disks the colours of the dominant wave-lengths are really mixed and the white light inherent in each case can be deducted.

Presents, February 19, 1891.

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February 26, 1891.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer and Vice-President,
in the Chair.

The Presents received were laid on the table, and thanks ordered
for them.

The Croonian Lecture was delivered as follows:—

CROONIAN LECTURE.—“On the Mammalian Nervous System;
its Functions and their Localisation determined by an
Electrical Method.” By FRANCIS GOTCH, Hon. M.A.,
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Physiological Laboratory, Oxford.) Received February 26,
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(Abstract.)

1. *Introduction.*

In the ‘Proceedings of the Royal Society,’ No. 273 (vol. 45, 1889,
p. 18), we published a preliminary account of some of the experi-
ments of which the results are given in detail in our full paper.

In that communication we stated that the object of our work then
was to endeavour to ascertain the character of the excitatory pro-
cesses occurring in nerve fibres when either directly, *i.e.*, artificially,
excited, or when in that state of functional activity which is due to
the passages of impulses along them from the central apparatus. The
most important way in which such a method could be applied was,
obviously, one which would involve the investigation of the excita-
tory changes occurring in the fibres of the spinal cord when the
cortex cerebri is stimulated. We must at once assume that the motor
side of the central nervous system is practically divisible into three
elements. (1.) *Cortical centres.* (2.) *Efferent* (pyramidal tract)
fibres, leading down through the internal capsule, corona radiata, and
spinal cord. (3.) *Bulbo-spinal centres* contained in the medulla and
the spinal cord, and forming the well-known nuclei of the cranial and
also of the spinal motor nerves.

It had already been determined, both by direct observation and by
the graphic method (1), that certain areas of the cortex were con-
nected with definite movements of various parts of the body, and (2)
that while the complete discharge of the cortical apparatus was

followed by a very definite and characteristic series of contractions of the muscles in special relation with the particular point excited, the effectual removal of the cortical central mechanism and subsequent excitation of the white fibres passing down through the internal capsule, &c., led to the production of only a portion of the effect previously obtained from the uninjured brain.

This method of observation in no wise showed what processes were actually occurring in the spinal and other nerve fibres, and although the ablation of the cortical centre to a certain degree suggested the extent to which the cortex acted, nevertheless it did not afford an exact demonstration of the same. Moreover, the data which the graphic method furnished were precluded, through their being muscular records, from determining what share, if any, the lower bulbo-spinal central nerve cells took, either in the production of the characteristic sequence of contractions or in the modification, whether in quality or in force, of the descending nerve impulses during their transit. It seemed to us that the only way to approach this subject would be to get, as it were, between the cortex and the bulbo-spinal system of centres. This would be accomplished if some means were devised of ascertaining the character of the excitatory processes occurring in the spinal fibres of the pyramidal tract when, upon excitation of the cortex, nervous impulses were discharged from cortical cells, and travelled down the cord.

The question as to the extent to which it is possible to obtain physical evidence of the actual presence in nerve fibres of excitatory processes, and thus to arrive at reliable data for the comparison of their amounts, is one which up to the present has been answered only indirectly, and that in two ways: first, by the extension of Helmholtz's classical experiment of determining the rate of transmission, and, secondly, by observing those variations in the electrical state of nerve fibres which Du Bois-Reymond discovered to be an invariable concomitant of the excitatory state. As will subsequently be shown in the historical retrospect, it is well known, through the researches of Du Bois-Reymond and others, that the fibres of the spinal cord, just as nerve fibres in the peripheral trunks, are characterised by showing, when unexcited, an electrical difference between their longitudinal surface and cross sections; and, furthermore, that when excited, a well-marked diminution of this resisting electrical state is produced in the fibres of the cord, as in those of nerve trunks. Now, since such excitatory variations in the electrical state are presumably parallel in time and amount with the presence in the nerve of the series of unknown processes, termed excitatory, which a series of stimuli evokes, it was reasonable to presume that, if the cortex were discharging a series of nerve-impulses at a certain rate down the pyramidal tract, there would be a series of parallel changes in the

electrical condition of the fibres in the cord tract, and that, with a suitable apparatus for responding to such changes, these might be both ascertained and recorded. The accomplishment of a further purpose, viz., the localisation of both paths and centres by ascertaining the excitatory electrical effects in relation with them, was one of the main objects we had in view. In carrying it out, we found it was unnecessary to employ the electrometer, and, in fact, that it was advantageous to use the galvanometer, the record of which would be more easily and more accurately noted, since its graduation admits of far higher magnification. Moreover, with this instrument it was possible, by employing a series of stimuli, of known number and duration, to obtain quantitative results of definite comparative value, as will be shown further on; and thus, to compare both the size of different central paths and the amount of nervous energy discharged along the same path from different sources.

The plan upon which the full paper is framed is, first, to give an historical retrospect of the work of authors who have opened up the study of electrical changes in the central and peripheral nervous system; second, to describe at length our mode of experimentation, with special reference to the modifications which we have introduced, then to compare roughly the results we have obtained by our present method with those which had been previously ascertained by the graphic method, and so introduce the description of the facts which we have discovered, elucidating the physiology of the spinal cord, both in its relation to the higher centres and to the peripheral nerves.

2. Experimental Procedure.

The observations were in all cases made on etherised animals (cat and monkey), with due regard to the special influence of the anæsthetic. The operative procedure was so designed as to provide for suitable exposure of a particular region of the nervous system for excitation, and of another part in which the electromotive changes evoked by the stimulation may be observed. The relative parts were as follows :—

Part exposed for Excitation..	Part exposed for Observation.
Brain (cortex and corona radiata)....	and spinal cord.
Do. do. do.	and mixed nerve.
Spinal cord	and spinal cord.
Do.	and mixed nerve.
Mixed nerve	and spinal cord.
Spinal roots	do. do.
Posterior roots	and mixed nerve.

The excitation was either electrical, chemical (*i.e.*, with absinthe and strychnine), or mechanical. In the former instance the duration

and intensity were specially determined. The records were made by a Thomson high-resistance reflecting galvanometer, and a Lippmann's mercurial capillary electrometer.

The tissue, whether nerve or spinal cord, was so arranged for observation as to be always suspended in the air, one end remaining in connexion with the animal; consequently any error due to current deviations from the rest of the body could only have a slight and unipolar effect.

3. *Resting Electrical Difference between the Cut Surface of the Tissue and its Uninjured Longitudinal Surface.*

The average amounts of this difference in the tissues observed were as follows:—

Cat.	Monkey.
Nerve (69 cases), 0·01 Daniell ...	(12 cases), 0·005 Daniell.
Root (5 cases), 0·025 „ ...	—
Cord (50 cases), 0·032 „ ...	(9 cases), 0·022 „

We have observed that the cord difference is greater when that tissue is in connexion with the higher centres, and that it rises after each excitation. An important fall of the difference is to be remarked in all three tissues as a direct result of systemic death.

4. *Electrical Changes in the Spinal Cord evoked by Excitation of the Cortex Cerebri and Corona Radiata.*

We further discuss in our full paper the following points additional to those described in our previous communication, and which have resulted from the observation of the above changes:—

- (a.) Localisation of cortical areas of representation in relation to the various regions of the cord.
- (b.) Bilaterality of representation in the central nervous system, as evidenced by the electrical changes in the two halves of the spinal cord, consequent upon excitation of the brain or cord.

5. *Electrical Changes in the Spinal Cord when Evoked by Direct Excitation of its Fibres, after Severance from the Encephalon.*

We have by employment of this method ascertained the proportionate existence of direct channels in the various columns of the spinal cord, our design embracing the quantitative comparison of the electrical changes (and so indirectly of the nerve impulses) which are transmitted as a result of minimal excitation of the fibres. To further control our observations on these points, we have also determined the

extent of interruption in any given channel by intervening sections of the same.

As an extension of this subject, we have investigated the concurrent spread of nervous impulses to collateral paths, and probably to centres, when this further condition is introduced by increase in the stimulus.

The above results have been obtained in the case of both ascending and descending impulses.

Among other general conclusions from this division of our research are the following :—

(1.) High degree of unilaterality of representation in the spinal cord.

(2.) Spread of impulses from one posterior column to another and from one posterior column to its neighbouring lateral column through centres.

6. *The Relation of the Paths and of the Bulbo-Spinal Centres in the Spinal Cord to the Peripheral Nerves and their Roots.*

We have investigated this important relationship in the following modes :—

(I.) *The Electrical Changes in the Spinal Cord evoked by Excitation of a Mixed Nerve or its Roots.*—The chief conclusions which have been deduced from the results of these experiments, by means of minimal excitation and the employment of the method of blocking by intervening sections, include the following :—

(1.) Complete obstruction offered to centripetal impulses reaching the cord by the central end of the anterior root.

(2.) Mode of conduction, direct and indirect, in the cord of centripetal impulses passing up the posterior root.

(3.) Localisation of the direct path of afferent impulses in the posterior column of the same side as that of the nerve or root excited.

(4.) Localisation of the indirect path of afferent impulses in the posterior columns of the same and the opposite side and the lateral column of the same side as that of the nerve excited.

(5.) Proportionate development of both systems of paths in the two sides of the cord.

Expressed in percentages of the total transmission, this proportion is as follows :—

Posterior column of same side as the excited nerve...	60 p. c.
Lateral column of same side as the excited nerve.....	20 „
Posterior column of opposite side to the excited nerve	15 „
Lateral column of opposite side to the excited nerve..	5 „

(II.) *The Electrical Changes in a Mixed Nerve or its Roots evoked by*

Excitation of the Spinal Cord.—Whereas in the foregoing series (I) we dealt only with ascending impulses, we proceeded to investigate the distribution of descending impulses by observing the above-named changes when the individual columns of the cord are excited by minimal and later with more intense stimuli, controlling our results by the method of intervening sections.

We can summarise the effects observed as follows:—

(1.) Marked quantitative diminution suffered by impulses, leaving the spinal cord by the anterior roots, whether originating in the cortex cerebri, corona radiata, or the lateral columns of the cord.

(2.) Localisation of direct transmission of impulses in the posterior column and passing out into the posterior roots of the same side.

(3.) Proportionate development of the direct and indirect paths in the individual columns of the cord, passing out into the mixed nerve of the one side.

(4.) Effects observed in the posterior roots when the bulbo-spinal centres are excited either by strychnine or electrically (kinæsthesia).

Finally, the chief general principles elucidated by this research may be stated as follows:—

(1.) Unilateral character of the representation of function in the paths of the central nervous system.

(2.) The physiological characteristics of the regions of a nerve centre:—

(a.) The kinæsthetic activity of the afferent region of the centre.

(b.) The obstruction offered by the efferent region, including the field of conjunction, to the transmission of impulses through the centre.

Presents, February 26, 1891.

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“The Rupture of Steel by Longitudinal Stress.” By CHARLES A. CARUS-WILSON. Communicated by Professor G. H. DARWIN, F.R.S. Received March 10,—Read March 27, 1890.

[PLATES 2, 3.]

In a paper read before the Royal Society on June 16, 1881, Professor G. H. Darwin stated: “It is difficult to conceive any mode in which an elastic solid can rupture except by shearing, and hence it appears that the greatest shearing stress is a proper measure of the tendency to break” (*Phil. Trans.*, 1882, p. 99).

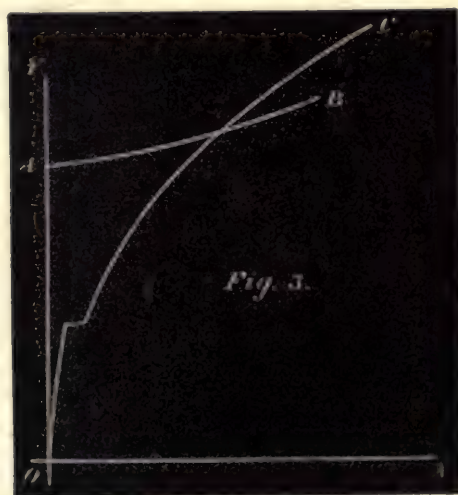
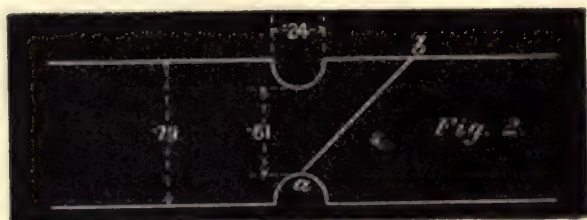
In this paper, I have recorded the results of some experiments made with a view to throwing light on the question raised by Professor Darwin.

The experiments were conducted in the mechanical laboratory at the Royal Indian Engineering College, Cooper's Hill, the machine used being a hundred-ton single lever hydraulic testing machine by Messrs. Buckton & Co., of Leeds. This machine is fully described in Unwin's ‘Testing of Materials of Construction,’ p. 133.

All tension experiments were performed on circular specimens fitted with a screw thread at each end, on to which were screwed steel nuts resting in spherical seatings to ensure directness of pull.

The shearing experiments were conducted on specimens screwed throughout their entire length into three steel blocks, and tested in double shear, *i.e.*, the two outside blocks were pulled in the opposite direction to the inside block—perpendicularly to the axis of the specimen, and the bar sheared in two places at once.

The idea of rupture necessarily implies an overcoming of resistance. If the power of steel to resist rupture under tension were a constant quantity, the conception of rupture would be simple, for we should only have to increase the stress up to the required amount, and the bar would break; but the resistance to rupture appears to be a function of the amount of flow that has taken place (see experiments described in Table II). If we take two axes, OX, OY, to represent respectively the elongation per unit of length, and the stress per unit of area of transverse section of elongated bar, we obtain the



true stress strain curve OC for the steel of which the bar is composed (fig. 3). We may refer to the same axes the curve AB showing the increase in the resistance to rupture with elongation per unit of length—and when AB cuts the curve OC the bar must break, *i.e.*, when the stress per unit of area of transverse section becomes greater than the breaking stress corresponding to that degree of elongation. If the two curves do not meet, the bar will continue to draw out—as is the case with lead.

The method that is ordinarily adopted for estimating the tensile

strength of a metal, viz., by dividing the maximum load by the original area, is a purely conventional method, and does not represent any real stress whatever; it simply shows what load would be sustained by a given section before it broke, and though it is no doubt a useful figure for engineers to know, it does not tell us anything about the actual stress at fracture; this can only be arrived at by dividing the load on the specimen at the point of rupture by the contracted area measured after the specimen has broken. In this paper, the breaking stress will always be measured in this way, and will be referred to as the "true tensile strength" of the metal. This is what M. Considère in his '*L'Emploi du Fer et de l'Acier*' (Paris, Dunod, 1885) calls "*résistance de striction*."

It is well known that when a bar is subjected to tension—the stress not being uniformly distributed, as, for instance, when the pull is not central—the mean stress borne by the bar at rupture is less than it would be if the stress had been uniformly distributed.

It is stated in Thomson and Tait's '*Natural Philosophy*,' Part II, p. 258, that "a solid of any elastic substance, isotropic or ælotropic experiences infinite stress and strain in the neighbourhood of a re-entrant edge or angle, when influenced by any distribution of force, exclusive of surface tractions infinitely near the angles or edges in question."

Three steel bars, numbered 829, 830, and 831, were taken and cut in three pieces; one piece was tested plain, and the second piece with a V-groove turned on it (see fig. 1). The tool cutting the V-groove was made with its cutting edges at about 90°, and the point as fine as possible. The results are given in Table I. It is clear that the V-groove is very prejudicial. For the same reason a V-groove with a rounded angle must be prejudicial, though not to such an extent, since the distribution of stress is more uniform. Specimen No. 834 was cut in three pieces and tested, one plain and one with a groove of the same shape as Nos. 829, 830, 831, but with the point of the cutting tool just rounded off. The strength of the grooved bar is now 0.95 of the plain. Similarly, with specimens 822 and 50, the grooved pieces have strengths of 84 and 89 respectively. These experiments show that the mean stress at rupture diminishes as the angle of the groove becomes more acute.

When, however, we come to test specimens with a groove as in fig. 2, we find that they are stronger than the plain specimens.

Table I gives the results of such experiments on seven steel bars (including the four already mentioned). Each bar was cut in three pieces.

The plain bar was tested first, and the groove then cut in the second piece to the same diameter as the contracted area in the plain piece, so as to secure as much as possible similarity of conditions.

Table I.

Laboratory No.	O.	V.	U.	O ₁ .	V ₁ .	U ₁ .	c.
829	73.5	53.1	83.2	100	72	113	53.2
830	73.4	57.8	87.0	100	79	119	50.5
831	70.4	58.0	84.6	100	82	120	50.0
834	59.5	56.7§	63.0	100	95	106	52.4
822	70.0	58.8§	77.5	100	84	111	52.5
439	60.5	—	74.5	100	—	123	36.4
50	58.1	53.7§	69.4	100	89	119	61.4

O, V, U are the tensile strengths of the plain, V-grooved, and U-grooved bars respectively in tons per square inch. O₁, V₁, U₁ the same, taking that of O as 100. c is the percentage contraction of area of the plain bar.

Fig. 1 shows the dimensions of the V-groove adopted in all cases, except in
 822 }
 Nos. 834 } §, where the corner was just rounded off by the cutting tool.
 50 }

Fig. 2 shows dimension of the U-groove.

It will be seen that in every case the U-grooved specimen is stronger than the plain, the average superiority being 16 per cent.

The effect of the U-groove by itself in producing non-uniformity of stress—as in the V-groove—would tend to make the U-grooved bar break at a lower stress than the plain bar, where the stress must be very nearly uniformly distributed, but, in spite of this prejudicial action, the U-grooved bar is the stronger.

This phenomenon is quite distinct from that mentioned by many writers, who have pointed out that a grooved specimen is stronger than a plain specimen of the same material—the stresses being reckoned in the conventional manner, viz., maximum load divided by original area (*cf.* Unwin, 'Testing of Materials of Construction,' p. 82, and Burr, 'Elasticity and Resistance of Materials of Engineering,' p. 230).

The reason of this is that the so-called tensile strength depends on the amount of drawing out before local contraction begins, and since in a plain bar the general contraction of area may be considerable, the actual load on the specimen, at the maximum, is much smaller than on a bar of the same metal in which the drawing out is suppressed—owing to the groove.

In the experiments quoted above, I have discounted altogether the contraction of area, and considered only the actual stress on the section at rupture.

It is possible that in some cases the metal at the groove is stronger

than at any other point in the bar; this would tell in its favour against a plain bar which is free to break at the weakest spot. The steel on which these experiments were conducted was very homogeneous, and the variation of strength within a short distance on the same bar would not be more than 4 or 5 per cent., so this would scarcely account for the phenomena.

On the other hand, a grooved specimen is at a disadvantage when compared with a plain specimen for four reasons :—

(i) The stress is much more unevenly distributed over the least section in the grooved bar than in the plain.

(ii) If the pull is not exactly parallel to the axis of the bar, bending stresses are induced, which are very prejudicial in the grooved bar, whereas their effect is largely neutralised in the plain bar by the ready flow of the metal.*

(iii) The load at rupture can be observed with accuracy in testing the grooved bar, for it breaks off short, and the required load is also the maximum load. In testing the plain bar, however, in consequence of the very rapid contraction of area immediately before rupture, the load has to be reduced, in order to keep the lever horizontal; sometimes the load cannot be run back quick enough, and the bar may break while the lever is resting on the bottom stop, so that too high a load may be observed as the load of rupture; this would tend to give a higher breaking stress in the plain bar than was actually the case.*

(iv) The grooved bar has a crystalline fracture. The plain bar has a silky fracture. Experiments will be quoted later on to show that the ultimate resistance to rupture is less, the more crystalline is the steel at the moment of rupture.

Careful measurements have been made of the test piece No. 831 (the others being very similar), to ascertain the least area of all planes passing through any point in the narrowest section at 45° to the axis (i) in the grooved bar, (ii) in the plain bar; the section of these planes is shown at *ab* in fig. 4; the diameter at the narrowest section was the same in both specimens. The ratio of the area of this plane in the grooved bar to that in the plain was found to be 183 : 100.

If, now, rupture is an overcoming of a resistance to shearing, the grooved bar ought to be stronger than the plain in the ratio of 183 to 100, other things being the same in both bars.

For the resistance to shearing, at rupture, will be the resistance of all the planes similar to those shown in the figures, equally inclined to the axis, and if the area of any one of these planes in the grooved

* This is only stated as a possible source of error; no result was accepted if there was any suspicion of its being thus influenced.

FIG. 4.



bar is 183/100 of any one in the plain, the resistances of all the planes will be in the same ratio.*

In this specimen (No. 831), the ratio of the true breaking stresses is actually 120 : 100. But there are four causes tending to reduce the strength of the grooved specimen, as has been shown above, viz. :— The non-uniformity of stress, the possibility of a pull not perfectly longitudinal, the danger of observing too high a load on the plain specimen, and, lastly, the crystalline nature of the steel at fracture in the grooved specimen.

* This assumes that the shearing stress is uniform over both of these oblique planes; the probability of this supposition is discussed in a paper on "The Distribution of Flow in a Strained Elastic Solid," published in the 'Philosophical Magazine,' for June, 1890.

All these causes tend to reduce the superiority of the grooved bar; the effect of the non-uniformity of stress is, no doubt, the greatest; in fact, as has been shown, if the groove is of V-shape, this causes the plain bar to be the strongest, so that it is quite conceivable that the non-uniformity of stress in the U-grooved bar, with other causes mentioned, might reduce the strength from 180 : 100 to 120 : 100 of the plain bar.

The following are the experiments referred to, in order to prove that the ultimate resistance of steel to shearing diminishes inversely with the drawing out:—

Six pieces of the same steel bar were taken and drawn out under tension to varying percentages of their length; they were then prepared as shearing specimens, as explained above, and tested in double shear, with the following results:—

Table II.

Bar. No.	Extension per cent.	Cont. of area per cent.	Area of section sheared.	Shearing stress at rupture.
			sq. in.	tons per sq. in.
1	0·0	0·0	0·317	20·50
2	5·0	4·8	0·322	20·10
3	10·0	9·1	„	20·21
4	15·0	13·0	„	21·77
5	20·0	16·6	„	22·12
6	20·2	16·7	„	22·37

Thus the ultimate resistance to shearing increases with the drawing out. Taking the contraction of area as a measure of the flow, No. 831 specimen contracted 50 per cent. on the plain bar and 28·1 per cent. on the U-grooved bar, so that the former would be at an advantage, compared with the latter, in this respect.

It appears, then, that the load that can be borne by a given section of steel without rupture can be increased by thickening the bar above and below the section in question, though, if the angle of the groove be too acute, the reverse is the effect. But the prejudicial action of a groove, owing to non-uniformity of stress produced, is the same in a U- or V-groove, and differs only in amount, so that the U-groove, by itself, could not strengthen the bar, but must weaken it; yet in spite of this, the U-grooved bar is stronger than the plain bar. The increase of strength, then, must be due to the added material; but in no way could such added material strengthen such a section, and enable it to stand a greater load, if rupture is produced by a certain intensity of tensile stress; we cannot diminish the mean stress on the section by thickening the bar above and below; on the

contrary, we increase the stress over part of the section, and it is the maximum stress that we have to reckon with, for the bar will begin breaking there and tear across.

There is no doubt that the added material increases the resistance to shearing, and I am, therefore, led to the conclusion that it is this increased resistance to shearing that causes the increase in strength; in other words, by adding material above and below the section, the shearing stress for elements lying in the section is certainly reduced, and, at the same time, the strength is certainly increased, the conclusion drawn being that the true measure of the tendency to break is the greatest shearing stress.

The fact that longitudinal tension is equivalent to a uniform dilating tension and a shearing stress, and that by the means above described we can diminish the latter, without altering the former, and thereby strengthen the bar, are strong reasons for supposing that Professor Darwin's statement is correct, and that it is the shearing stress produced by longitudinal stress that causes rupture.

If it be true that the rupture of a steel bar under tension is determined by the greatest shearing stress, we should expect to find that a definite relation existed between the ultimate resistance to direct shearing and the same to direct tension.

Much has been written about the relation of these two resistances, but the conclusions drawn are very misleading, since the tensile strength considered has been that calculated in the conventional manner, which has, as has been shown, no real significance and is no real stress.

By a well known theorem, the greatest shearing stress is equal to one-half the longitudinal stress; we should then expect to find that one-half the true tensile stress at rupture was equal to the stress at rupture in a shearing experiment on a piece of the same steel.

If the steel be soft, it will contract locally before breaking; hence the greatest shearing stress will be less than half the longitudinal stress in the ratio of the sections of a cylindrical and contracted bar, cut by planes parallel to dc and ab (fig. 5) respectively, the two bars having the same cross section at coe ; in other words, in the ratio of $\sqrt{2}$ (area across coe) to (area across aob), where $boe = 45^\circ$. If this ratio be called θ , and the true tensile stress at rupture be p , the shearing stress at rupture in a shearing experiment should be equal to $\frac{1}{2}p\theta$.

Table III gives the results of some experiments made to investigate this question. The tensile and shearing experiments were made respectively on pieces of steel cut from the same bar; the former were made on circular specimens screwed at each end, and resting on nuts bearing on spherical seatings; the shearing specimens were screwed

FIG. 5.



along their entire lengths, and tested in double shear in screwed steel blocks to eliminate bending.

The 3rd column gives w_1 , the original section of the tension specimen; the 4th gives c , the percentage contraction of area in the

TABLE III.

Laboratory No.	Original Dimensions.	w_1 .	c .	$\frac{1}{2}p$.	θ .	$\frac{1}{2}p\theta$.	w_2 .	s .
801	$1\frac{1}{4}'' \times 1\frac{3}{8}''$	0.542	43.7	32.5	0.88	28.6	0.305	28.7
900	"	0.628	46.3	32.8	0.87	28.5	1.021	30.1
42	1" round	0.400	61.7	26.2	0.81	21.2	0.322	22.7
43	"	0.389	64.0	27.9	0.79	22.0	0.322	22.4
970a	"	0.381	54.1	37.1	0.81	30.0	0.322	31.2
970b	"	0.386	53.4	36.3	0.83	30.1	0.322	31.0
898a	"	0.384	53.1	37.9	0.83	31.5	0.312	31.8
898b	"	0.384	50.5	37.6	0.83	31.2	0.312	31.2
198	"	0.348	33.9	31.2	0.93	29.0	0.322	30.6
199	"	0.348	50.5	34.2	0.85	29.3	0.322	28.6
49	1 $\frac{1}{4}$ " round	0.640	56.5	22.1	0.82	18.1	1.021	18.3

[Note.—Those bracketed together are from the same bar. Nos. 198 and 199 each give the mean of two experiments on the same bar. Nos. 801, 900, 970a and b, 898a and b, 198, and 199 are from steel bars prepared by the Patent Nut and Bolt Company, the steel being made by the Barrow Steel Company. Nos. 42 and 43 are from bars of soft crucible steel manufactured by Messrs. Osborn at the Clyde Steel Works, Sheffield. No. 49 is from a bar of Lowmoor iron.]

tension experiment; the 5th gives half the true tensile stress; the 6th gives the values of θ , deduced by measurement in each case; the 7th gives $\frac{1}{2}p\theta$; the 8th gives w_s , the area of the specimen in the shearing experiment; and the 9th column gives s , the intensity of shearing stress at rupture in the shearing experiment.

It will be seen that in every case $\frac{1}{2}p\theta$ is very nearly equal to s , i.e. the shearing stress at rupture in a tensile experiment is very nearly equal to the ultimate resistance to shearing in a pure shearing experiment.

There are, however, two points to be considered before accepting the result of these experiments.

The distribution of stress over the section of rupture in the tension experiment has been assumed constant, whereas it is not actually so. I find, by actual measurement, that the area of a plane section at 45° to the axis passing through the centre of the narrowed section bears to the area of a parallel plane passing through a point on the circumference, the ratio of 100 to 108, in a bar which has contracted 50 per cent., so that the shearing stress is rather greater at the centre, and hence the value of $\frac{1}{2}p$, given above, is too small by about 4 per cent.

On the other hand, it has been pointed out to me by Professor Darwin that the distribution of stress in the shearing experiment is probably not uniform, being greater in the neighbourhood of the application of the stress.

Experiments were made with two pieces of Lowmoor iron, cut off the same bar, and prepared as shearing specimens in the ordinary way, and tested in double shear, one with an area to be sheared of twice 1.039 square inch, and the other of twice 0.322 square inch. The result was as follows: Large section, shearing stress at rupture—(i) 18.7, (ii) 18.9; mean, 18.8 tons per square inch. Small section, stress at rupture—(i) 20.1, (ii) 20.6; mean 20.35. Giving the latter as 8.2 per cent. stronger than the former. The smaller the section the more uniform will be the stress, and with the small section employed in the experiments quoted in Table III the stress is probably nearly uniform.

It would seem, then, that the possible errors due to the unequal distribution of stress in the tensile and shearing experiments would nearly balance one another, and that we may regard these results as tending to confirm the theory that the greatest shearing stress is the proper measure of the tendency to break.*

* I have made experiments of a similar kind on cast iron. Great care was taken in casting to secure uniformity, by casting the bars upright and cutting off the spongy top; they were cast with two heads which were turned to fit spherical seatings. The shearing specimens were cut off bars from the same cast. The bars in tension were 10 inches long between the shoulders, and turned throughout their length.

It will now be necessary to enquire how far the appearance of the fracture of a steel bar affords evidence of its having broken by shearing.

In a bar of circular section and uniform thickness throughout its length, every plane at 45° to the axis opposes an equal resistance to the tangential stress caused by direct tension. Hence, there is no one plane or planes along which the bar would be more ready to break by shearing than along any other plane, provided that the material was of uniform strength throughout. If, however, the bar be gradually thinned at a certain point, this will no longer be the case; it has been shown on p. 252, that the area of a plane at 45° to the axis passing through the centre of the narrowed section is less than the area of a plane passing through any other point in that section; hence, there will be a surface formed by a complete cone of 45° , with apex at the centre of the narrowed section, which will oppose a less resistance to rupture by shearing than any other similar cone with apex at any other point. This cone is shown in section in fig. 6 at *gof—aab*. We should then expect to find rupture result in a fracture formed by a cone and crater, or, since there is nothing to determine along which part of the cone rupture will take place, we may expect to find the cone irregularly broken up, part on one end, and part on the other.

This narrowing of girth at one point always accompanies the rupture of soft steel, and we invariably find such a cone and crater; figs. 5 and 6, Plate 2, and 6 and 7, Plate 3, are good examples.

[*Note*.—The rupture of cast iron in compression by shearing is of course well known. Fig. 2, Plate 2, shows the cone of shearing very well.]

In flat bars of soft steel, this shearing action is still more marked. Here the surface of the least resistance is a plane at 45° to the axis and making 90° with the thin side of the bar; it is evident that in a bar whose width is considerable compared with its thickness, and which has suffered considerable local contraction, this plane has the least area of all planes at 45° to the axis passing through any point in

	Sectional area.	Breaking stress. Tons per sq. inch.	Mean ditto.
Tension....	{ 1·047 0·980	10·40 10·40	10·40
Shearing ...	{ 0·317 0·327	6·14 4·78	5·46

The ratio of the former to the latter breaking stress being 1·9.

The mean crushing stress was 41·5 tons per square inch; diameter of specimen, 0·875 inch; length, 1·5 inch.

In the 'Proceedings of the Institution of Civil Engineers,' vol. 90, p. 406, Messrs. Platt and Hayward give results of shearing and tensile tests of cast iron, from which it appears that the ratio of the breaking stresses is 2·2.

FIG. 6.



the contracted section. The result is, that in flat bars of soft steel the fracture is almost invariably as shown in figs. 4 and 5, Plate 3. Fig. 8, Plate 3, shows a flat bar of soft steel just about to rupture by shearing along a plane $\perp$ the width of the bar—resulting no doubt from an accidental weakness in that direction.

In the cases considered above, the steel has been of sufficiently uniform quality to allow of the fracture taking place over a surface of least resistance to shearing; but, unless this condition be fulfilled, the form of the fracture will be quite different.

Figs. 7 and 8, Plate 2, show the fracture of a brass bar where the plane of least resistance to shearing has been determined by a punch mark (opposite the arrow) on the surface. Fig. 1, Plate 2, shows a steel bar where the apex of the cone is at the circumference, owing to the presence of a weak spot there.

Every fracture is caused by the presence of a more or less well defined weak spot; the stress is greatest at this spot, and the material tends to tear in a plane $\perp$ the axis passing through this spot. This tearing action can be observed by drilling a small hole in a steel plate, and straining it. The plate pinches in near the hole, and gives way first on each side of the hole, and then tears right across. The experiment may be stopped before the tear has reached the sides.

When the steel is hard, this tearing continues in the plane in









which it commenced, *i.e.*, perpendicular to the axis; but when the steel is soft, the plane of the tear gradually tilts over and coincides with the surface of least resistance to shearing, *i.e.*, becomes inclined at 45° to the axis.

Now, at rupture, an originally soft bar is harder in the centre of the narrowed section than at the circumference, where the drawing out has been less; hence, fracture commences at the centre perpendicular to the axis, and tears outwards until it reaches the softer material, when it will continue along a surface of least resistance to shearing, *i.e.*, along a surface formed by the intersection of two cones. Hence, we find the fracture of a soft steel bar consisting of a crater with a more or less extended base; see figs. 5 and 6, Plate 2, and 6 and 7, Plate 3.

The harder the steel, at the outset, the broader will be the base of the crater, until, in very hard steels, there is only a rim or crown left round the edge; and in the hardest steels all trace of the surface of least resistance to shearing disappears.

[*Note.*—I have employed the term “hard” in the sense usually understood, *i.e.*, where the “hardness” is measured by the value of the limit of elastic resistance.]

“Photometric Observations of the Sun and Sky.” By
WILLIAM BRENNAND. Communicated by C. B. CLARKE,
F.R.S. Received October 30,—Read December 11, 1890.

1. In the publications of the Society from 1859 to 1870, many communications by Sir Henry Roscoe on this subject will be found. Of these, the most important bearing directly on my observations are—

a. Bunsen and Roscoe, “On the Direct Measurement of the Chemical Action of Sunlight,” in ‘Phil. Trans.,’ 1863, pp. 139–160.

It is proved, *inter alia*, that equal shades are produced in photographically sensitised paper by equal products of intensity of light $\times$ time of insolation. The preparation of a photographic paper which shall always possess the same degree of sensitiveness is carefully described.

b. Roscoe, “On a Method of Meteorological Registration of the Chemical Action of Total Daylight,” in ‘Phil. Trans.,’ 1865, pp. 605–631 [Bakerian Lecture].

The law is stated, *inter alia*, that light of intensity 50 acting for 1 second has the same effect as light of intensity 1 acting for 50 seconds.

The mechanical arrangement for exposing the paper horizontal, or by the aid of a vertical drum, is explained.

Tables are added of half-hourly readings at Manchester, giving general actinic effects for different seasons of the year, &c.

c. Roscoe and Baxendell, "On the Relative Chemical Intensities of direct Sunlight and diffuse Daylight at different Altitudes of the Sun," in 'Roy. Soc. Proc.,' vol. 15, 1866-67, pp. 20-24.

By "total daylight" is meant the whole resultant action of the Sun and sky on paper exposed horizontally.

By "diffuse daylight" is meant the same action when the Sun was stopped out.

The "direct sunlight" was taken as the difference between these two; it does not appear to have been observed directly.

d. Roscoe, "On the Chemical Intensity of Total Daylight at Kew and Pará," in 'Phil. Trans.,' 1867, pp. 555-570.

e. Roscoe and Thorpe, "On the Relation between the Sun's Altitude and the Chemical Intensity of Total Daylight in a Cloudless Sky," in 'Phil. Trans.,' 1870, pp. 309-316.

2. My observations made at Dacca, in 1861-1866 (repeated at Milverton, in Somersetshire, during the last year), were made in entire ignorance of the work of Sir H. Roscoe; his results, therefore, so far as they agree with mine, afford an independent support to my theory. My experiments have been directed largely to ascertaining the laws of the distribution of the actinic power in the sky, and thus the work of Sir H. Roscoe overlaps mine at particular points only. So also Roscoe has taken numerous observations of the sky more or less clouded; I take no observation except when the sky is clear, as I find even a very slight haze to produce large differences in the measurements, and to bring into the numerical results complications that I have not at present attempted to deal with.

3. The method of measurement I adopted, is the darkening produced in sensitised photographic paper; for this effect I accept Roscoe's term of "the chemical action." My method of measurement differs from that of Roscoe in one important point: I use strips cut from one uniform sheet of ordinary photographic paper; all my measurements are so far relative, and I obtain the same numerical results (ratios) with any paper. I compare ultimately the effect of the Sun and of a candle on this same paper. Roscoe, by preparing special paper with definite proportions of nitrate of silver, &c., depends on thus reproducing paper of exactly the same sensitiveness. I make each measurement numerically (as did Roscoe) by comparing the shade produced with some standard blackness.

4. I assume that in the burning of a stearine candle, the "chemical action" is proportional to the material consumed. I have taken as my unit (*i*) of measure of chemical action, the darkening produced at a distance of 1 inch from the wick of the candle, when 100 grains were consumed, which, in the candle I used in India, occupied about

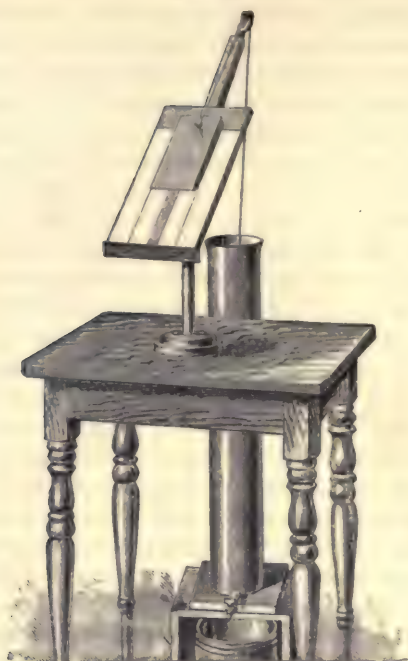
47 minutes. [I am here narrating the course I pursued in commencing these observations at Dacca; I very soon discarded the candle, as I was able, by the aid of my table given below, to recover the unit of measurement by a Sun observation.]

5. I form a strip of photographic paper about $\frac{3}{4}$ inch deep into a circular ring, placed inside a metal cylinder 3 inches in diameter. I place now my standard candle eccentrically, at a distance of 1 inch from the surface, and burn the 100 grains of stearine. I thus get a strip which is gradually coloured from the point nearest the centre (where the intensity is unit i) to the most remote point (where the intensity is $\frac{1}{4} i$). By calculating the distances of various points of the ring from the wick, the intensities corresponding to these distances can be marked. I exhibit a small strip (of somewhat different dimensions) so calibrated to show a scale of intensities; it has lost its original shade in consequence of fixing and toning. For actual purposes of measurement, a strip is used in its original unfixed state.

6. My earlier observations on the chemical action of the Sun and sky, were made in Bengal, with a "mica actinometer." In this, small squares of one sheet of sensitised paper were covered by 1, 2, 3, 4 . . . thicknesses of mica cut from the same plate; the sheet of paper then exposed to any light for a certain time gave me a series of chequered shades. To measure the effect of the Sun or of any portion of the sky, I noted the time necessary to darken the paper till it matched one of the squares in blackness. This instrument I have long since laid aside, as I have superseded it by better; but by its aid in 1863 I was led to the attempt of measuring the chemical action of the Sun, in a clear sky, for each degree of the Sun's altitude, so as to form a table of constants, which would render a direct reference to the candle power unnecessary.

7. I have made an instrument (fig. 1) similar to one employed in India. The plane on which to expose the sensitised paper has motions in altitude and azimuth; a perpendicular style is placed at the corner; and, by shifting the plane until the style casts no shadow, the plane can be adjusted at right angles to the Sun's rays, and the Sun's altitude can be read by a brass Gunter's quadrant. A slide which covers the strip of sensitised paper, is made to move uniformly up the plane, by means of a string passing over a pulley attached to a float in a column of water in a long cylinder (the one used in India was a rain-gauge); the float descends as the water is drawn off by a stopcock at the bottom of the cylinder. Lines can be drawn on a gauge pasted on the plane, beside the longitudinal slit, in which is exposed the sensitised paper, corresponding to the motion for 1, 2, 3, . . . 20 seconds; also a second gauge has been drawn for a larger tap giving quicker motion. By simply moving the sensitised paper

FIG. 1.



laterally, a fresh portion of it is brought under the longitudinal slit, and the observation can be immediately repeated, several times if so desired.

8. By comparing the darkening produced in the paper in paragraphs 5 and 7, we easily show that we have to expose the paper four times as long to produce the effect caused by diminishing the distance one half; and that a light of intensity 4 acting for 1 second has the same effect as a light of intensity 1 acting for 4 seconds. This I think might have been assumed; Bunsen and Roscoe, in their paper (1863) above cited, have, however, taken great pains to prove it.

9. My early experiments were designed to test the total effect of the sky and Sun for photographic purposes. I have always experimented mainly by exposing the paper at right angles to the Sun's rays. Roscoe on the other hand, exposes his paper in a horizontal plane. It will be seen below, that theoretic considerations have led me to another method of observation, which gives directly the measure of effect really desired, and does not require a clear heavens down to the horizon on all sides (the Octant Actinometer). I give as a first example of my experiments the following table (A). The observation

was taken on 21st December, 1863, on the roof of my house at Dacca, the sky being perfectly clear. The paper was exposed at right angles to the Sun, thus giving the effect of the Sun, together with the total effect (resolved on the plane at right angles to the Sun) of that portion of the visible sky within 90° of the Sun.

Table A.

Sun's altitude.	Time of observation.	Number of seconds per inch in motion of slide.	Length in inches of strip for constant shading, C.	Chemical action measured in unit I.
11° 0'	7 ^h 41 ^m A.M.	11·0	1·52	0·06
14 0	8 2	12·0	1·2	0·07
19 0	8 31	6·2	1·8	0·088
24 46	9 17	6·0	1·26	0·132
29 0	9 33	7·25	0·74	0·186
32 0	9 51	6·7	0·77	0·192
34 30	10 12	6·0	0·82	0·203
36 48	10 26	6·7	0·68	0·219
39 0	10 54	6·5	0·683	0·226
41 30	11 22	6·5	0·625	0·24
41 40	11 35	6·7	0·59	0·2525
42 20	11 50	6·7	0·56	0·266
42 30	12 0	6·25	0·6	0·269

The number in the fifth column in this table, is the reciprocal of the product of the two numbers in the third and fourth columns.

Thus, taking the last but one observation,

$$6\cdot7 \times 0\cdot56 = 3\cdot752,$$

and
$$\frac{1}{3\cdot752} = 0\cdot266.$$

The constant C of shading used as the standard of comparison was the tint produced in the same paper by the candle burning 47 minutes at 1 inch distance. Hence, the unit I here employed was 47×60 times *i* the unit in paragraph 4 above.

In order to get a deeper shade of darkening in the first two observations, when the Sun was low, a smaller stopcock was used than in the succeeding observations.

In each of these observations, the actual velocity of the slide was observed by an assistant with a watch. As explained in paragraph 7, this constant can be obtained more easily and exactly by a gauge, pasted on the plane beside the slit, graduated for the stopcock used.

10. The observations of Table (A), and numerous other similar observations, were taken with great care, the strips being read the same evening. The strips taken on separate days, were also compared with each other; it was thus found that the numerical values for the chemical action were the same, with different paper, and with different candles. In England, I have, within the last two years, made similar observations to those at Dacca twenty-five years ago, and I submit three of the strips taken; these have been fixed, and have consequently changed both in density and in colour, and are submitted merely for explanation. The photographic sensitised paper, now prepared in England, keeps in the dark for months unchanged, and renders constant reference to the candle standard unnecessary. But by the aid of the table (B) which immediately follows, I could always in 1889 and 1890 recover the standard unit, by an observation of the Sun better than from the candle.

11. The chemical action of the Sun alone, is got in a dark room, by arranging a vertical slit, so that the Sun's light falls exactly down the strip of paper, which I expose at right angles to his rays. To get the chemical action of the Sun and sky (*i.e.*, the portion of the visible sky within 90° of the Sun) together, the exposure is completely in the open. The chemical action of the sky (*i.e.*, the resultant action on the plane at right angles to the Sun of that portion of the visible sky within 90° of the Sun) is got by an exposure in the open, a vertical stick having been arranged so that its shadow should just cover the exposed strip.

As I took each of these three kinds of observations, giving numerical results α , β , γ respectively, I was enabled from the simple formula $\alpha + \gamma = \beta$ to check my observations, to test the closeness with which the strips could be read certainly, and to show again that an intensity of 4 acting for 1 second has the same effect as the intensity of 1 acting for 4 seconds.

12. I found, as Roscoe, working in a less pure atmosphere, found in a still greater degree, that observations very close to the horizon were not to be depended upon. Also, in the cold weather at Dacca, at which season alone the sky was sufficiently clear, the Sun did not attain a greater altitude than about 45° . The flat roof of my house offered nearly a complete hemisphere of unclouded blue; nevertheless, I know that the full effect of the band of sky near the horizon, must have been to some extent interfered with by haze; and the constants in some of the tables that follow will be, in a small degree, affected by this cause.

13. The following Table (B) is shortened, from one which I drew up and printed photographically at Dacca in 1865. It represents the mean result of very numerous observations, taken at altitudes of the Sun between 5° and 45° . From 45° to 90° , the table has been

partially completed by using the formula $i = 0.494 (0.991)^e$ where e is the distance traversed by the Sun's rays in the atmosphere for different altitudes. This formula is parallel to the equation $t = Ap^e$ used by Pouillet in his memoir on the Solar Heat (and can be found in 'Taylor's Memoirs,' vol. 4, p. 49).

N.B.—In this table, in each observation the sensitised paper was exposed at right angles to the Sun's rays: so that a different portion of the sky was observed at each altitude.

Table B.
Chemical Action of Sun and Sky.

Sun's altitude.	Sun alone.	Sky alone.	Sun and sky together.	Sun's altitude.	Sun alone.	Sky alone.	Sun and sky together.
5...	0.0064	0.0125	0.0189	31...	0.1120	0.0636	0.1739
6...	0.0090	0.0156	0.0246	32...	0.1134	0.0643	0.1777
7...	0.0120	0.0189	0.0390	33...	0.1158	0.0648	0.1806
8...	0.0156	0.0224	0.0380	34...	0.1185	0.0654	0.1839
9...	0.0196	0.0256	0.0453	35...	0.1215	0.0660	0.1875
10...	0.0238	0.0288	0.0526	36...	0.1238	0.0665	0.1903
11...	0.0283	0.0319	0.0602	37...	0.1262	0.0670	0.1932
12...	0.0330	0.0349	0.0679	38...	0.1290	0.0675	0.1965
13...	0.0377	0.0376	0.0782	39...	0.1307	0.0678	0.1985
14...	0.0423	0.0401	0.0824	40...	0.1331	0.0683	0.2014
15...	0.0471	0.0425	0.0896	41...	0.1349	0.0686	0.2035
16...	0.0519	0.0447	0.0966	42...	0.1369	0.0689	0.2058
17...	0.0566	0.0477	0.1043	43...	0.1384	0.0692	0.2076
18...	0.0612	0.0486	0.1098	44...	0.1407	0.0695	0.2103
19...	0.0657	0.0594	0.1161	45...	0.1429	0.0700	0.2128
20...	0.071	0.0520	0.1221				
21...	0.0750	0.0529	0.1279				
22...	0.0786	0.0549	0.1335	50...	0.1504	0.0711	0.2215
23...	0.0826	0.0562	0.1388	55...	0.1568	0.0720	0.2288
24...	0.0865	0.0574	0.1439	60...	0.1620	0.0727	0.2347
25...	0.0905	0.0583	0.1488	65...	0.1662	0.0732	0.2394
26...	0.0939	0.0595	0.1534	70...	0.1696	0.0736	0.2432
27...	0.0974	0.0604	0.1578	75...	0.1721	0.0739	0.2461
28...	0.1008	0.0613	0.1621	80...	0.1737	0.0741	0.2478
29...	0.1040	0.0621	0.1661	85...	0.1747	0.0742	0.2489
30...	0.1070	0.0628	0.1698	90...	0.1751	0.0743	0.2494

14. It must be carefully noted, with respect to these older observations, that what I actually observed, was the number of seconds' exposure at each altitude necessary to produce a particular darkening in the sensitised paper, viz., the shade produced by the constant candle at distance unity, in that particular paper; the numbers printed

were obtained by taking the inverse of these times for the chemical action.

15. The table is only a first approximation; yet I have much greater confidence in the values, than in those given by any one observation, the table itself being deduced from a very large number of experiments.

Sir H. Roscoe believes (Bakerian Lecture, 1865) that he brought the errors due to matching shades to within 2 per cent. correct; and in graduating strips, the mean error was found by him not to exceed 1 per cent. of the measured intensity. I am not satisfied that my separate observations were always so closely accurate in the matching of shades. I employed my daughters independently, to match shades, and compared them with my own reading, and found that the readings *sometimes* differed more than 2 per cent.

The photographic paper employed, varied somewhat in tint; that exposed to the candle being a little redder than that exposed to the Sun and sky; the same intensity in the darkening was sought in every case. I suppose the difference in tint to have been due to the heat of the candle.

16. The effect of the sky observed, was that due to the effect of each elemental area of it multiplied by the sine of the angle between that elemental area and the normal to the plane of exposure, these infinitesimal effects being summed throughout the visible sky within 90° of the Sun.

The chemical action of the sky (*i.e.*, of the portion of it thus included) is seen to be half that of the Sun at 45° altitude; and at altitudes of the Sun below 13° , where little more than half the sky is included in each observation, to be greater than that of the Sun.

17. I found the chemical action of the Sun, exactly the same for the same altitude, at all seasons of the year and at all hours of the day, as far as the experiments went at Dacca, and I find in Somersetshire the same chemical action of the Sun at the same altitude as at Dacca. I have not been able to get exactly the same candle that I used at Dacca; and a difference in the composition of the stearine might possibly cause a small difference in the results, but I believe not one of much importance.

[The observations in Table J below in the postscript show that the difference is absolutely nil.—27th October, 1890.]

In the 'Phil. Trans.' 1867, pp. 558—562, Roscoe says that for equal altitudes of the Sun the chemical intensities are equal; and he "assumes" that the same "relation between the Sun's altitude and chemical intensity holds good at Kew, Heidelberg, and Pará." These results of Roscoe are confirmed by my observations; he obtained them only by "averaging" numerous observations taken at Kew, and assuming that the effects of cloud, &c., in the long run were self-destructive.

Roscoe supposes that a marked difference which he found in intensity between spring and autumn might be due to a difference in transparency. I can only explain some of Roscoe's results by supposing that the sky was not perfectly clear at the time of the observations. Indeed, from the description, many of Roscoe's observations would appear to have measured the effects of cloudiness rather than of Sun and sky. I have no anomalies in the results of my observation except such as I think I may fairly attribute to cloud or haze. My experience in England is that it requires months of watching to catch a sky that will give results similar to those I obtained regularly in Dacca during the cold season.

In the 'Phil. Trans.,' 1867, p. 559, Roscoe finds (by the same method of "averaging") that "the relation between the Sun's altitude and the chemical intensity of total daylight is graphically represented by a right line." And in the 'Phil. Trans.,' 1870, p. 315, Roscoe and Thorpe say that the relation between altitude and total chemical intensity, for altitudes above 10° , is seen to be accurately represented by a straight line.

Table B indicates, and Table G below proves, that the straight line is only a first approximation to the truth. The calculation from my Table B of the chemical action of the whole visible sky (and Sun) on the horizontal plane can be effected, as shown further on in the present paper.

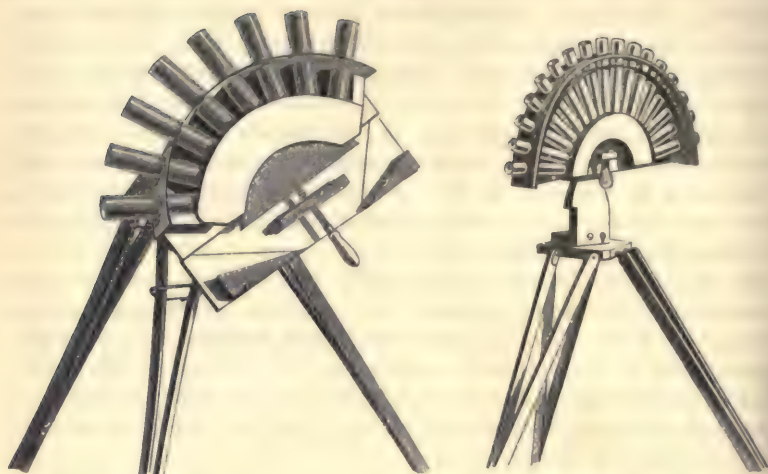
18. Various observations had led me to expect that the chemical action of the sky at the same moment was different in different parts of it. To investigate this suspicion, I designed an instrument which I call the *Mitrailleuse Actinometer* (fig. 2); I place in the President's hand photographs of two of these instruments.

I mount a number of similar cylindric tubes in one plane in a semi-circle, to the centre of which each tube is directed. One extremity of each tube lies on the circumference of the circle; the other extremities lie on a concentric circle of about half the radius. In the circumference of this smaller circle, is a semicircular series of holes, against which a semicircular block, carrying the sensitised slip of paper, is pressed by a screw. Each cylinder in the first Dacca mitrailleuse cut out of the sky a circle of $8^\circ 28'$ angular diameter. One of the tubes near its top, carries a small plate of wood, on which stands a style parallel to the tube, by means of which the particular tube can be brought into a line with the Sun. By another motion the plane of all the tubes can be adjusted to the plane of symmetry (or elsewhere).

[A vertical plane through the Sun at any time divides the visible sky into two exactly similar portions. I will call it the plane of symmetry].

19. The observations (Table C) were taken 23rd December, 1864,

FIG. 2.



at Dacca (among many other similar observations taken in the same cold weather), in the plane of symmetry. The barrels of the mitrailleuse were fixed 10° apart, the altitude of the Sun being $42^\circ 28'$.

Table C.

Altitude of the axis of the barrel of the mitrailleuse.	Distance of axis of barrel from the sun = θ .	Observed chemical action during six minutes' exposure = $i\theta$.	Calculated value of $i\theta$ from $i\theta = 0.12 \operatorname{cosec} \theta$.
10°	$-32^\circ 58'$	0.2	0.2205
20	$-22^\circ 58'$	0.6	0.3075
30	$-12^\circ 58'$	0.7	0.5348
40	$-2^\circ 58'$	..	2.3186
50	$+7^\circ 2'$	0.844	0.98
60	$17^\circ 2'$	0.322	0.4097
70	$27^\circ 2'$	0.188	0.264
80	$37^\circ 2'$	0.184	0.1992
90	$47^\circ 2'$	0.177	0.164
100	$57^\circ 2'$	0.144	0.143
110	$67^\circ 2'$	0.14	0.1304
120	$77^\circ 2'$	0.128	0.123
130	$87^\circ 2'$	0.122	0.1201
140	$97^\circ 2'$	0.12	0.1209
150	$107^\circ 2'$	0.128	0.1255
160	$117^\circ 2'$	0.136	0.1347
170	$127^\circ 2'$	0.136	0.1563

The readings of the chemical action are taken in terms of the unit of candle power, and were compared also with a graded Sun-strip, made at the same time from the same photographic paper by the water-motion actinometer, fig. 1.

The observations given by the barrels at 170° and 10° are too low, doubtless owing to haze so near the horizon. No observation could be made with the barrel at 40° , because the Sun could not be kept out of it. The observation made by the barrel at 20° is (apart from comparison with computed value) evidently erroneously large. I give the table as an early observation that shows well that there is a point of minimum sky intensity at 90° from the Sun. It also appears that if i_a represent this intensity for the altitude α of the Sun ($= 0.12$), then the intensity of the sky at a point θ from the Sun is given (roughly only according to this table) by the formula

$$i_a \operatorname{cosec} \theta.$$

This observation was made in the plane of symmetry: it turns out that the value, $i_a \operatorname{cosec} \theta$, gives the intensity very accurately, in whatever plane θ be measured from the Sun.

I would note once more that my observations are largely comparative, and the results obtained are independent of the unit: it is not necessary to reduce the readings in this table to the one-second unit.

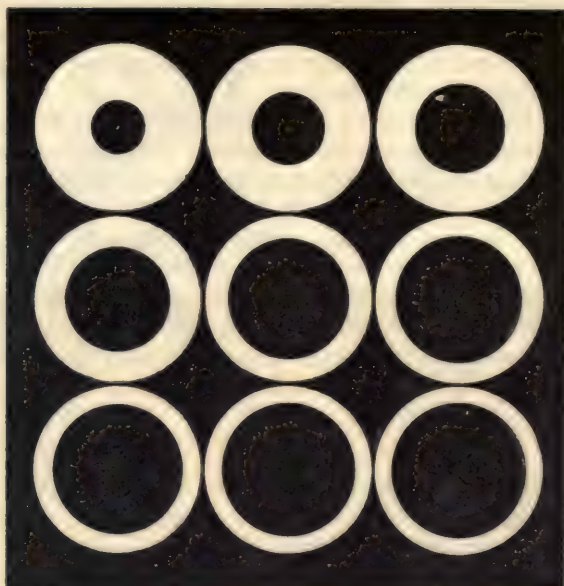
20. For any altitude of the Sun (α), the chemical action of the sky is a minimum at all points of a great circle 90° from the Sun, the plane of which is the plane of minimum intensity (i_a). And at this moment, the chemical action of the sky at any point distant θ from the Sun is given with great accuracy by the formula

$$i_a \operatorname{cosec} \theta.$$

As the whole of the mathematical developments of this paper are founded on this law, I have been careful not only to verify it by numerous observations both at Dacca and in Somersetshire, but also to vary the form of the observations in every way I could devise.

21. Thus, the mitrailleuse has been placed in the plane of minimum intensity: in this case, all the barrels give accurately the same reading, except that those barrels 10° from the horizon read rather lower, as I anticipated they would; there must nearly always be *some* haze near the horizon.

Next, the mitrailleuse was placed at various angles with the plane of symmetry, by turning it round the line joining one of its tubes with the Sun. The observed chemical actions agree well with $i_a \operatorname{cosec} \theta$. Next, by means of stops, I made the aperture of each barrel of a mitrailleuse to be $c \sin \theta$, where θ is the distance of the axis of the barrel from the Sun. This mitrailleuse being exposed,



the barrel with aperture $c \sin \alpha$ being directed to the Sun, the circular darkened spots were found to be very accurately of uniform depth. Further, I calculated the times of exposure, for a (particular) mitrailleuse which ought, on the law $i_\alpha \operatorname{cosec} \theta$, to give a uniform tint. I exposed this mitrailleuse for these calculated times, first in the plane of symmetry, afterwards in a plane inclined to it at an angle of 52° ; the results agreed closely with my anticipation, and show $i_\alpha \operatorname{cosec} \theta$ to be a very good approximation.

22. I have therefore made full use of the expression $i_\alpha \operatorname{cosec} \theta$ for the chemical action of the light of the sky in a circle distant θ from the Sun (whose altitude is α).

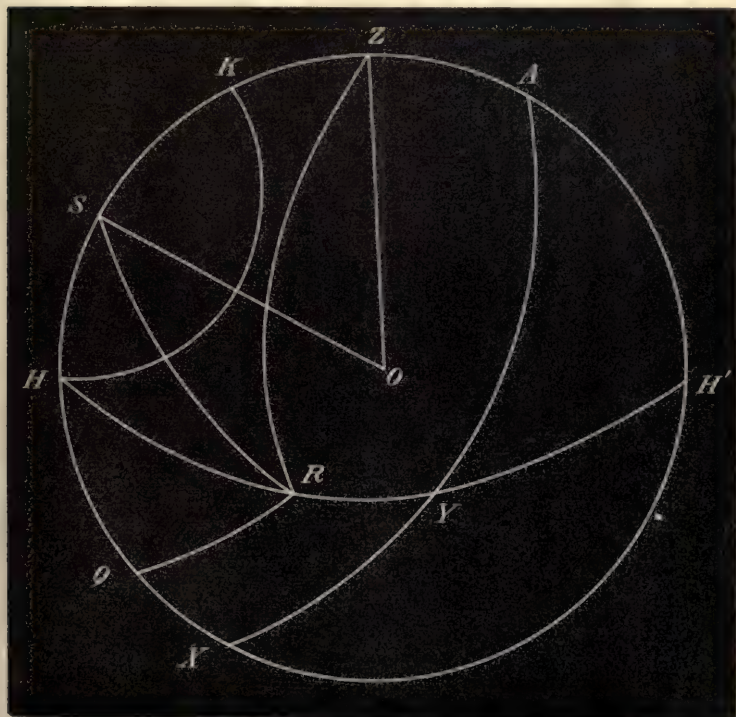
In carrying out integrations which include the portion of the sky actually occupied by the Sun, we do not, by employing this formula, introduce any infinite expression; for each circular band of the sky of small breadth $d\theta$ distant θ from the Sun has an area $2\pi \sin \theta d\theta$; the chemical action of such band is therefore $2\pi i_\alpha d\theta$: so that the total chemical action thus attributed to the sky in the area occupied by the Sun's disk would be inappreciable.

23. Bunsen and Roscoe ('Phil. Trans.,' 1859, p. 891) determined chemically the action of the rays falling from a measured portion of cloudless sky situated near the zenith, and then compared the *visual* luminosity of this same portion of zenith sky with that of the total heavens. They say "the amount of light chemically measured,

which falls from the same surface of zenith sky, multiplied by the preceding ratio, must give the chemical action which the whole sky would produce on a horizontal unit of surface."

I have below in one or two points only attempted to institute a numerical comparison between the results of Sir H. Roscoe and my own; considering the great difference in our methods, I am not surprised that no good coincidence in the results can be established.

DIAGRAM 1.



24. Having given i_a the chemical action in the circle of minimum intensity, to calculate the total chemical action of the sky on a plane exposed at right angles to the Sun.

(N.B.— i_a is a constant for this calculation, but it varies with α the altitude of the sun).

Let the figure (Dia. 1) represent a projection on the plane of symmetry, S being the Sun, Z the zenith, HRYH' the horizon, AYO the plane of minimum intensity, SH = α the Sun's altitude.

Let θ be the angular distance from the Sun of the elementary zone

QR, and ϕ the angular distance of an element in the zone QR from SQ.

Denote by I_s the chemical action exerted by a circular area s of the sphere, on the plane at right angles to the Sun.

The area of an element will be $d\phi d\theta \sin \theta$, the intensity of the chemical action will be $i_a \operatorname{cosec} \theta$.

The angle between the normal to the element considered and that to the plane AYX is θ .

$$\therefore d \cdot I_s = d\phi d\theta \sin \theta \times i_a \operatorname{cosec} \theta \times \cos \theta$$

$$\therefore I_s = i_a \int_0^{2\pi} \int_0^\theta d\phi \cdot d\theta \cdot \cos \theta = 2\pi i_a \sin \theta.$$

Or, for the whole hemisphere, of which the Sun is the pole,

$$I_H = 2\pi i_a \dots\dots\dots (Q),$$

from which, to get our desired result, we have to subtract the chemical action I_g of the gore XYH.

$$I_g = i_a \int_a^{\frac{\pi}{2}} \int_{-\operatorname{RSH}}^{\operatorname{RSH}} \cos \theta \cdot d\theta \cdot d\phi = 2i_a \int_a^{\frac{\pi}{2}} \operatorname{RSH} \cos \theta \cdot d\theta.$$

$$(\cos \operatorname{RSH} = \tan \operatorname{SH} \cot \operatorname{SR} = \tan \alpha \cot \theta).$$

$$\begin{aligned} \therefore I_g &= 2i_a \int_a^{\frac{\pi}{2}} \cos^{-1} (\tan \alpha \cot \theta) \cos \theta d\theta \\ &= 2i_a \left[\operatorname{Lim}_{\alpha}^{\frac{\pi}{2}} \{ \cos^{-1} (\tan \alpha \cot \theta) \sin \theta \} - \int_a^{\frac{\pi}{2}} \frac{\tan \alpha \cdot d\theta}{\sqrt{(1 - \sec^2 \alpha \cos^2 \theta)}} \right] \\ &= 2i_a \left[\frac{\pi}{2} - \int_a^{\frac{\pi}{2}} \frac{\tan \alpha \cdot d\theta}{\sqrt{(1 - \sec^2 \alpha \cos^2 \theta)}} \right]. \end{aligned}$$

Whence, subtracting this from equation (Q),

$$I_H - I_g = i_a \left\{ \pi + 2 \int_a^{\frac{\pi}{2}} \frac{\tan \alpha \cdot d\theta}{\sqrt{(1 - \sec^2 \alpha \cos^2 \theta)}} \right\}.$$

This expression cannot be integrated in finite terms, but, by using a formula of reduction in series, it gives

$$I_H - I_g = i_a \left\{ \pi + 2\pi \frac{\sin \alpha}{1 + \sin \alpha} \left[1 + 0.25 \tan^4 \left(\frac{\pi}{4} - \frac{\alpha}{2} \right) + 0.14 \tan^8 \left(\frac{\pi}{4} - \frac{\alpha}{2} \right) \dots \right] \right\},$$

which is the formula I have used in numerical computations. $I_H - I_g$ is the numerical value in the column headed "Sky alone" in

Table B, which is thus brought into direct verification with i_a , observed by the mitrailleuse.

An example of the actual calculation of i_a is added in Appendix B, not for publication.

25. The values for $I_{II} - I_G$ for different altitudes of the Sun in Table B are much the most trustworthy observations, and are the means obtained from a very large number of observations. I have, therefore, by the formula obtained in the last paragraph (24), inversely calculated the value of i_a for every 5° within the limits 5° to 40° , and placed them in Table D.

Table D.

Sun's altitude.	i_a calculated from Table B (column headed "Sky alone").
5°	0.00329
10	0.00681
15	0.00928
20	0.01073
25	0.01144
30	0.01188
35	0.01205
40	0.01218
45	0.01213
50	0.01209
55	0.01204
60	0.01200
65	0.01195

26. *Theorem.*—On the resolution of the chemical action of the sky in a direction perpendicular to any plane.

The figure (Dia. 2) is supposed an orthographic projection of the visible hemisphere on the plane of the horizon; S being the Sun, Z the zenith, HSZM the projection of the plane of symmetry, M'MI that of the plane of minimum intensity, and M'SI that of the plane through S at right angles to each of the other planes (which I call the plane of the Sun's altitude). These three planes, when produced, divide the sphere into eight quadrantal surfaces, of which SMI is one. In the quadrantal triangle SMI, S, M, I are the poles of the opposite sides.

Let the polar coordinates of P (an element of the surface) be $PSZ = \phi$ and $SP = \theta$. Then, as before, the element will have an area $d\phi \cdot d\theta \cdot \sin \theta = i_a d\phi \cdot d\theta$.

Let the planes OSM, OSI, and OIM (O being the centre of the hemisphere) be taken as coordinate planes; OS, OM, OI, the three axes

whence

$$Q_{(s)} = i_a \sin \alpha \iint d\phi \, d\theta \cos \theta + i_a \cos \alpha \iint d\phi \, d\theta \sin \theta \cos \phi$$

$$= 2i_a (\pi \sin \alpha + 2 \cos \alpha) \dots\dots\dots (X).$$

29. This is a mere literal result: what is required is (Q_z); *i.e.*, the chemical action of the hemisphere about Z resolved on the horizontal plane; that is, the relation between the chemical action in the plane of minimum intensity i_a (Sun's altitude α) and the "total chemical action of diffused daylight" as observed by Roscoe on horizontally exposed paper.

The answer (Q_z) is identical in form with (Q_s) as given in equation (X) above; but the limits of θ are functions of ϕ which lead to elliptic integrals.

Referring back, however, to diagram 1, it will be seen that (Q_z) differs from (Q_s) by the addition of the gore AYH, the subtraction of the gore HYX; which will be found to be no difference at all; as the values of the chemical action of each element in the subtracted gore are equal to those for a corresponding element in the added gore, with the same sign and angles of resolution on the horizontal plane. Hence we must have—

$$(Q_z) = (Q_s) = 2i_a (\pi \sin \alpha + 2 \cos \alpha) \dots\dots\dots (Y).$$

As this is a result of the first importance, I submit at the end of this paper in an Appendix, not for publication, the work by which I first arrived at the equation (Y) by laborious transformation of the elliptic integrals, which are reduced finally so that two terms, each irreducible by integration in algebraic form, destroy each other.

30. The results thus arrived at by employing the law of the cosecant are so neat that a suspicion may arise that the law may have been assumed as one lending itself to mathematic manipulations.

I may be permitted, therefore, to state, that the law was arrived at, more than twenty-two years ago, by experiment simply, and the subject soon after laid aside. The present mathematic investigations were only recommenced within the last two years, in order to institute a comparison between my old Dacca observations and those of Sir H. Roscoe.

31. In 'Phil. Trans.,' 1870, p. 314, Sir H. Roscoe gives a table showing the total chemical action of diffuse daylight (*i.e.*, of the whole sky, the Sun being stopped off) on horizontally exposed paper. These observations were taken in Portugal, with a perfectly clear sky, and I therefore select them for comparison with the foregoing theory and observed values of constants.

Columns 1 and 2 are copied from Roscoe, *l.c.*; column 2 gives my $Qz = 2l_a (\pi \sin \alpha + 2 \cos \alpha)$. In column 3 I give l_a , calculated from this equation. In column 4 I place the values of i_a obtained from table B, the "sky alone" column, by the aid of the formula at the end of Art. 24.

In the 5th column the values in column 4 are brought up by proportion for comparison with those in column 3, taking the observation at altitude $42^\circ 13'$ as the best; *i.e.*, increasing all the numbers in the ratio of 121 to 160.

Table E.

1. Sun's altitude.	2. Diffuse daylight.	3. i_a calculated from 2.	4. i_a calculated from Table B.	5. Values in Column 4 brought up.
$9^\circ 51'$	0·038	0·0078	0·0068	0·0090
19 41	0·062	0·0105	0·0107	0·0141
31 14	0·100	0·0150	0·0118	0·0156
42 13	0·115	0·0160	0·0121	0·0160
53 9	0·126	0·0170	0·0121	0·0160
61 8	0·132	0·0177	0·0120	0·0159
64 14	0·138	0·0187	0·0120	0·0159

The discrepancies do not appear at first sight great between the results of Sir H. Roscoe and my own. But his observations would show the maximum value of i_a attained when the Sun was at or near the zenith, mine that this maximum occurs when the Sun is about 45° or 50° altitude.

It is true that in the Dacca Table B, the actual observations extend only to 45° or thereabout, and that the values for altitudes of the Sun above 45° are only filled in hypothetically; but my best established observations at Dacca, for altitudes of the Sun from 30° up to 45° , show directly that at altitudes of the Sun of 45° or 50° the value of i_a would reach a maximum.

In my Dacca observations, each additional 5° to the Sun's altitude brings into effect an additional 5° gore of the sky. It is therefore clear (apart from the law $i_\theta = i_a \operatorname{cosec} \theta$ and the integrations consequent thereon) that i_a will have a maximum value when α , the Sun's altitude, is about 50° or 60° .

I am not surprised that so considerable a discrepancy results from a comparison of the observations. In a single series of observations, the incidental errors of reading, &c., would introduce into the small numbers given in column 3 sufficient differences to alter entirely the law indicated for i_a .

32. Since in table E the value of i_a for $\alpha = 42^\circ 13'$ is found from Roscoe's observations to be 0.016, from mine to be 0.012, it follows that Roscoe's unit of chemical action is $\frac{4}{3}$ of my Dacca candle unit. This is merely a first attempt to correlate these units.

33. The resultant chemical action of the sky on a horizontally exposed piece of paper, the Sun's altitude being α , is found

$$= (2\pi \sin \alpha + 4 \cos \alpha) i_a.$$

This vanishes when

$$2\pi \sin \alpha + 4 \cos \alpha = 0,$$

$$\text{i.e., when} \quad \tan \alpha = -\frac{2}{\pi},$$

$$\text{or} \quad \alpha = -32^\circ 29'.$$

This gives an absolute value for twilight, supposing daylight to cease when the diffused daylight of Roscoe entirely vanishes.

The extreme limit at which twilight has been certainly observed is when the Sun was 24° below the horizon; at which time the formula $i_a(2\pi \sin \alpha + 4 \cos \alpha)$ would show the chemical action of diffuse daylight to be only $\frac{1}{40}$ part of what it was just after sunset.

In other words, the formula

$$i_a(2\pi \sin \alpha + 4 \cos \alpha)$$

gives a very good agreement with the observed duration of twilight, supposing, that is, the illumination and the chemical action to follow much the same laws in this extreme case.

34. Taking up the expressions for U, V, W at the end of Art. 26, I integrate them for the octant of the sphere contained by the three coordinate planes, viz., the plane of symmetry, the plane of minimum intensity, and the plane of the Sun's altitude; i.e., I take ϕ and θ each from 0 to $\frac{1}{2}\pi$; which gives

$$[U] = \frac{\pi}{2} i_a, \quad [V] = [W] = i_a.$$

This suggested the construction of the octant actinometer, which requires only one-fourth of the visible sky to be clear for observation, and gives the value of i_a directly, requiring no calculations of reduction.

35. The *octant actinometer* (fig. 3) consists of three quadrantal planes, MOS, MOI, and IOS, joined at their edges so as to form a hollow trihedral, and mounted so that one of the edges, OS, can be brought to point to the Sun, and the plane MOI will then coincide with the plane of minimum intensity. The instrument has another adjustment

FIG. 3.



by which it can turn round OS as an axis, and if one of the planes MOS, IOS be brought to coincide with the plane of symmetry, the other will coincide with the plane of the Sun's altitude.

I take a small square (diagram 3) of sensitised paper and cut it along CO; then, slipping the part COB under AOC, so that B coin-

DIAGRAM 3.



cides with C, it forms a rectangular trihedral of sensitised paper. This is placed in a small exposure trihedral of cardboard, and covered by a thin metallic trihedral in the trihedral angle of the octant. (I make several of these trihedrals of sensitised paper, so as "in the field" to take quickly a series of observations.)

The trihedral of sensitised paper is, of course, carefully covered up till the instrument is in adjustment; if then exposed to the action of the sky for (say) 30 seconds, the readings on the quadrantal planes MOS and IOS will be each $30 i_a$, and that on the quadrantal plane MOI will be $30 \cdot \frac{1}{2}\pi \cdot i_a$.

36. I tried this octant actinometer on the 13th August, 1890—the first day that the sky had been partially clear for a long time—and also (with a more imperfect sky) on the 15th and 16th August, 1890, at Milverton, near Taunton. The exposures were all for 30 seconds. I give the whole results.

Table F.

Time.	Sun's altitude.	[V] = [W]. <i>i</i> on the two planes.	[U]. <i>i</i> on the third plane.
1890. 12th Aug., 5.0 P.M. ...	21° 30'	0·0187	0·024
5.10 ...	19 30	0·0183	0·025
5.20 ...	18 0	0·0191	0·027
5.33 ...	16 15	0·0191	0·023
5.42 ...	14 30	0·0183	0·027
5.47 ...	13 30	0·0150	0·023
15th Aug., 11.25 A.M. ...	52 30	0·0270	0·030
11.30 ...	53 0	0·0240	0·027
16th Aug., 0.45 P.M. ...	52 15	0·0170	0·019
0.50 ...	52 0	0·0190	0·028
4.0 ...	29 0	0·0170	0·020
4.15 ...	27 30	0·0150	0·019

It is evident that these observations were interfered with greatly by haze or cloud; but it may be well to explain exactly how they were taken.

A "sunstrip" was shaded first by the water-motion actinometer; the altitude of the Sun being known, the value of any line in this sunstrip, in terms of the Dacca candle unit, was known by the aid of Table B.

The adjustment and working of the octant actinometer were found not difficult. The readings in the two planes [V] and [W] were found practically equal; the results are in the third column. These "readings" were obtained directly by comparison with the "sun-

strip," and divided by 30 are the numbers in column 3. Similarly, the numbers in Column 4 represent [U].

$$\text{Now } [V] = [W] \text{ should be } \frac{2}{\pi} \cdot [U].$$

These observations do not give [U] large enough.

Also, the observations of 12th August would show the value of i_a when $\alpha = 20$ to be about 0.018 or 0.019. But the Table D shows the true value of i_a when $\alpha = 20^\circ$ to be 0.0107; that is to say, the readings of 12th August, 1890, with the octant actinometer are altogether too high. This may easily be so without any fault in the instrument or error in the observations, and on two reasons. First, the presence of any bright cloud may have given the readings [V] and [W] too large. (Bunsen and Roscoe, in 'Phil. Trans.,' 1859, p. 905:—"These observations prove that the presence of a thin film of cloud increases the amount of chemical illuminating effect in the most striking manner."—The clouds "act as mighty reflectors of light.") Secondly, a very slight haze over the Sun would give the sunstrip too low, and thus largely increase the results of columns (3) and (4) read by it.

I do not consider these observations to decide anything as to the merits of the octant actinometer, which can only be satisfactorily tested by the sky of Dacca or some similar subtropical or tropical station.

37. It is difficult to determine which method of resolution of the sky and Sun gives the most useful measure of the general total effect, whether for determining the time of exposure of a photographic plate or for estimating the effect on vegetation. Sir H. Roscoe has taken (for the sky) the resultant action on paper exposed horizontally; I append, therefore, in Table G the chemical action similarly measured, so that column 2 is exactly = the "diffuse daylight" of Roscoe, and column 4 = the "total daylight" of Roscoe. This table is deduced by calculation from the Dacca Table B, by the aid of the law $i_\theta = i_a \operatorname{cosec} \theta$, i.e., from the value for $I_B - I_G$ in Art. 24, and the value $Q_z = 2i_a (\pi \sin \alpha + 2 \cos \alpha)$, which are directly derived from that law.

This table, as far as $\alpha = 45^\circ$, is a direct consequence of the Dacca observations. The values given from 50° up to 60° are a theoretical extension, perhaps as near as would be given by interpolation between known extremes. I do not think the numbers for α from 60° to 90° , which might be arrived at in a similar way, would have any real value.

This table, equally with the Dacca Table B, shows how large the sky effect is in comparison with the Sun effect, especially for altitudes of the Sun below 30° . This may be the explanation of the reason why trees close to the north side of greenhouses exercise a prejudicial influence.

Table G.—Showing Chemical Action of the Sun and of the whole Sky, resolved on the Horizontal Plane, for various Altitudes of the Sun (the Sky being perfectly clear from Cloud and Haze).

1. Sun's altitude.	2. "Diffuse daylight" of Roscoe.	3. Sun $\times$ sine alti- tude.	4. Sum of columns 2 and 3.
5°	0·0150	0·0006	0·0156
10	0·0343	0·0041	0·0384
15	0·0510	0·0122	0·0632
20	0·0625	0·0240	0·0865
25	0·0718	0·0382	0·1100
30	0·0784	0·0535	0·1319
35	0·0830	0·0697	0·1527
40	0·0865	0·0856	0·1721
45	0·0882	0·1010	0·1892
50	0·0893	0·1149	0·2042
55	0·0900	0·1285	0·2181
60	0·0893	0·1403	0·2296

The present paper contains my Dacca experiments and numbers arrived at by calculation therefrom. I have been for a year making similar experiments in England whenever the sky by its clearness offered any chance of a good observation; but I have not been able to get any observation such that I should attempt to correct the Dacca Table B thereby. I, therefore, am satisfied to publish the present paper in its present form, leaving to others its extension by the help of further observations under a perfectly clear sky.

38. *Postscript*, 15th October, 1890.—I have within the last few days made a number of observations with the octant actinometer, and have also, by making a few sunstrips at different altitudes, compared the times for the candle unit with those of the Dacca tables. These observations, though giving no numerically valuable results, strongly confirm the views I have expressed in this paper, and I append a statement of them.

On the 10th October the sky was seemingly clear; but, the values obtained for $[V] = [W]$ being much too high, I did not continue the observations.

On the 11th October I took a sunstrip at 12^h 8^m, the Sun's altitude being 31° 30'; comparing this afterwards with a candlestrip, I found the time for the candle unit to be 8·5 seconds. Referring to the Dacca table, I found the time for the same altitude, 31° 30', to be 8·9 seconds. I therefore used this sunstrip for the observations in the preceding table. I infer that, at least at 12^h 8^m, the sky on the 11th October was really clear. Some of the values in this table are higher than those obtained by computation for i_a .

Table H.—Octant Observations at Milverton, Somerset, 1890.

Time.	Sun's altitude.	[V] = [W].	[U].
10th Oct., 11 ^h 15 ^m	31° 0'	0·0237	0·0293
12 19	31 45	0·0225	0·0270
2 11	25 10	0·0130	0·0171
11th Oct., 12 10	31 15	0·0226	0·0312
1 8	29 30	0·0150	0·0250
1 44	27 30	0·0120	0·0298
2 13	24 20	0·0120	0·0208
3 9	19 0	0·0104	0·0208
3 43	14 30	0·0094	0·0156
4 22	8 45	0·0052	0·0073
5 9	Sunset.	0·0013	0·0033

On the 13th October, 1890, I made a series of octant observations; but, as I doubted whether the sky was really clear (*i.e.*, as the clear sky of Dacca in the cold weather), I made a series of sunstrips, as under:—

Table J.

Time.	Sun's altitude.	Exposure for candle unit (Milverton).	Exposure for same (Dacca).
1890. 13th Oct., 11 ^h 35 ^m ...	30° 15'	9·0 secs.	9·25 secs.
12 0 ...	30 30	9·2	9·2
12 33 ...	29 45	9·4	9·4
1 5 ..	28 45	0·75	9·75
1 35 ...	27 45	16·0	10·0
2 13 ...	23 30	14·75	12·0
2 54 ...	19 15	19·0	15·0

In the first four observations, the sky was apparently, and doubtless really, clear; in the three latter observations, some slight invisible cloud over the Sun produced great changes in the sunstrips.

From the exact coincidence in the readings in the four first observations, at Dacca and Milverton, I think it follows (1) that there was no material difference in my candles at Dacca and Milverton; (2) that the chemical action of the Sun at the same altitudes was the same at Dacca and Milverton.

It is also clear that the number of really fine hours of sky in England (*i.e.*, when it can be compared with the Dacca cold-weather sky) is very small—perhaps not a score in the year. And further that, in a great many apparently clear skies in England, there is

present some haze, visible or invisible, that affects the readings of the chemical action on sensitised paper very largely, even to 50 per cent.

The octant observations, the intensities estimated on the *first* of the above strips, are as follows:—

Table K.

Time.	Sun's altitude.	[V] = [W].
1890. 13th Oct., 12 ^h 4½ ^m	30° 45'	0·0268
1 8½	28 30	0·0267
1 39	27 30	0·0251
2 18½	23 15	0·0244
2 59	18 30	0·0216
3 24	15 45	0·0194
4 11½	9 45	0·0055
4 48	3 0	0·0041

The values of [V] = [W], being so much greater than I expected, led me to imagine that, though the sky was apparently clear, the observations might have been affected by the hygrometric state of the atmosphere. There had been a fog in the morning, and the air was, though translucent, saturated all the afternoon.

The next day, the 14th October, was a similar day (fog in the morning), and I commenced octant observations much earlier in the morning. The results are given in the following table:—

Table L.—Octant Observations, 14th October, 1890.

State of sky.	Time.	Sun's altitude.	[V] = [W].	[U].
Clear sky, but slight fog faintly to be seen.	9 ^h 38 ^m A.M.	22° 48'	0·0217	0·0217
	10 8	25 15	0·0235	0·0254
	10 39	27 30	0·0272	0·0272
	11 2	29 0	0·0217	0·0326
	11 32	29 45	0·0217	0·0326
	12 1 P.M.	30 45	0·0272	0·0399
	12 30	30 0	0·0290	0·029
Light fleecy clouds in the sky octant.	1 0	29 0	0·02540	0·03990
	1 30	27 30	0·03080	0·0435
Clear, but still faint clouds.	2 19	24 0	0·02720	0·02720
Clear.....	2 45	21 0	0·0210	0·03290
Partial clouds	3 15	17 15	0·0163	0·0355
	4 0	11 30	0·0091	0·0127

The effect of the faint fog in increasing the value of $[V] = [W]$ is plainly seen in the morning observations. The effect also of a very few faint fleecy clouds is seen in the increase of $[V]$ and of $[U]$ for the observations at 1^h 0^m and 1^h 30^m, before which no clouds had been visible. The air was saturated the whole day.

The candles which I used in all these observations, were the "Belmont Sperm," supplied to me so as to burn 100 grs. in 47 minutes.

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	12 1 P.M.	30 45	0·0272	0·0399
	12 30	30 0	0·0290	0·029
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The candles which I used in all these observations, were the "Belmont Sperm," supplied to me so as to burn 100 grs. in 47 minutes.

"On the Minute Structure of the Muscle-Columns or Sarcostyles which form the Wing-Muscles of Insects. Preliminary Note." By E. A. SCHÄFER, F.R.S. Received December 15, 1890,—Read January 8, 1891.

[PLATES 4 & 5.]

The fibres of the wing-muscles of most insects are made up of readily separable longitudinal elements, which are often called the "wing-fibrils," although several observers have remarked the existence of an apparently fine fibrillation in them. To avoid ambiguity, I shall employ the term "muscle-columns" (*Muskel-säulchen*, Kölliker), or its equivalent "sarcostyles,"* to designate these elements. They are united together to form the fibres by a not inconsiderable amount of granular interstitial substance (*sarcoplasm*, Rollett). This substance has been regarded (Ramón y Cajal) as the true contractile material of the muscles, but it is easy, nevertheless, to observe the contraction of the sarcostyles, isolated in white of egg, a fact which has been pointed out by more than one writer on the subject (Merkel, Kölliker).

If an insect of which the wing-muscles are of the character above described is cut open and placed in alcohol of about 90 p.c. for twenty-four hours or more, and is afterwards transferred to glycerine, the sarcostyles of the wing-muscles can be isolated and examined without difficulty; they exhibit almost every phase of extension and retraction (or contraction), and the usual appearance of alternate dark and light transverse bands, with a fine line traversing each light band. When stained with dyes, such as hæmatoxylin, the dark bands are found to take the staining most intensely; the fine transverse lines are much less stained, and the clear bands hardly at all. The various parts of the sarcostyle evidently differ from one another in their behaviour to staining reagents, and the transverse striation is not to be explained by the effect of the varicosities of the sarcostyle upon the light transmitted

* Σάρξ, flesh, στύλος, a column.

through it (Haycraft); moreover, many of the sarcostyles show no such varicosities. A more valuable, because more sharply selective, method of staining is that recommended by Rollett ('Wien. Akad. Denkschr.,' vol. 51) for alcohol-glycerine muscles. This consists simply in the after-application of the gold-formic method to the tissue. In place of treating the *fresh* muscle with chloride of gold and afterwards with formic acid, the *alcohol* muscle, which has been afterwards steeped in glycerine, is taken. If *fresh* muscles are thus treated, the sarcoplasm alone is stained, the sarcostyles remaining colourless (or they may be entirely dissolved by the action of the formic acid). In this way, in the leg-muscles, the often-described appearance of a network is obtained. But if the *alcohol-glycerine* muscle be taken, the reduction of the metal takes place *in the sarcostyles*, and almost exclusively in their dark bands, so that, while the interstitial sarcoplasm and the clear bands of the sarcostyles remain clear and colourless, the dark bands of the sarcostyles are deeply coloured of a tint varying from an intense purple-red to a faint purple-blue. Rollett recommends the application of this method to the study of the structure of the leg-muscles, but it is still better applicable to that of the wing-muscles, since it brings out in them, with a clearness which renders the application of the photographic method comparatively easy, points of structure which, up to the present, with the usual methods of investigation, have remained obscure.

Before describing the special points which are thus capable of elucidation, it is necessary to adopt names for the several parts of the sarcostyle. For the more or less cylindrical disk which forms the dark band I shall retain the name "sarcoous element," without thereby intending to imply that it accurately corresponds to the part to which that name was originally applied by Bowman; in a general sense, I believe that it will be found to do so. The term represents, on the whole, the *Querscheibe* of the German, the *disque épais* of French, authors. The fine line which bisects the light band I shall term "transverse membrane" (*Quermembran*, Krause; *Zwischenscheibe* of German authors; *disque mince* of French writers). The light space separating the ends of the sarcoous elements from the transverse membranes may, for the present, be simply spoken of as the "clear interval;" it corresponds with the isotropous substance of authors. The segment of a sarcostyle comprised between two transverse membranes may be termed "muscle-segment" or "sarcomere" (*Muskelkästchen*, Krause).

The relative amount of the sarcomere occupied by its several parts varies with the degree of extension or retraction (? contraction) of the tissue. In the retracted condition (figs. 1 and 1a) the sarcostyle, which is relatively thick and moniliform, appears formed almost

entirely of the sarcous elements, which are distinctly bulged, and are arranged closely succeeding one another with but narrow clear intervals between them. In these very narrow clear intervals the laterally-stretched and thinned-out transverse membranes cannot be seen unless the sarcous elements are forcibly dragged somewhat apart in the process of isolating them; if this is done the transverse membranes become visible (figs. 2 and 2a). In moderately extended sarcostyles (fig. 5) the sarcous elements are more separated from one another, the clear intervals being correspondingly longer and the transverse membranes distinct. In greatly extended sarcostyles (figs. 3 and 3a) the sarcous elements are not only lengthened and much narrowed, but show a tendency to separate in the middle into two halves, leaving a space between them. The clear intervals are also lengthened, and the transverse membranes are thickened; the whole sarcostyle being narrowed. It may be inferred, from the separation of the sarcous element, that it is really constituted of two halves, which in the retracted fibre abut against one another in the middle of the muscle segment, but in the extended fibre are separated from one another. Indications of this separation can be made out even in the non-extended sarcous element, as in some of those represented in fig. 8. Whether or not there is a fine membrane between the two halves, as described by Hensen, my preparations do not enable me to determine. Nor have I been able to observe in them the further separation, with still further extension, of separate disks (accessory disks, *Nebenscheiben*) from the ends of the sarcous elements, a separation which has been described and figured by several good observers.

In the preceding statements and descriptions there is nothing that is altogether novel or that has not been described with sufficient clearness by previous authors. But the application of photography leaves no room whatever to doubt the accuracy of those descriptions.

There is, however, one essential point of structure which I have only seen clearly in preparations made by this method, and which is also distinctly shown in the photographs. Various authors (Wagener, Krause, Kölliker, van Gehuchten), as before said, have described a fibrillation of the sarcostyles of the wing-muscles; or at least a longitudinal striation of the sarcous elements, with a dotted appearance of the transverse membranes. This striation is very plain in several of those wing-sarcostyles which are here photographed, and also in all others which are similarly prepared. It is even plainer under the microscope than in the photographs, because the mass of red-stained substance which forms the sarcous elements allows hardly any actinic light to reach the photographic plate, and the sarcons elements, when well stained, look, therefore, nearly uniformly black on the positive. It is very difficult, however, to trace the longitudinal striations

through the clear intervals under the microscope, and I was at first disposed to believe that it was confined to the sarcous elements. But the first photograph which was taken showed faintly, but unmistakably, that it extended also through those intervals. This can also be detected at certain parts of those photographs which are here reproduced. The longitudinal striation, therefore, although by far the most marked in the sarcous elements, extends through the whole length of the sarcostyle. It might, therefore, be supposed to represent a fibrillation of the sarcostyle, and this is the view which has been taken by all previous authors who have noticed the appearance. They have supposed the muscle-column to be constituted of a number of juxtaposed fibrils, each of which is composed of successive alternations of the substance composing the sarcostyle, each one, therefore, being composed, in the middle of each segment, of a rod-like portion of the sarcous element; at either end of this, and continuous with it, of a portion of the substance of the clear interval; and, lastly, at the ends of the segments, of a portion of the transverse membrane. The sarcous element is, according to this view, formed by the juxtaposition of a number of rod-like elements, which are stained by hæmatoxylin and similar methods (amongst which must be reckoned this alcohol-gold method); the clear intervals being formed of continuations of these rod-like elements, which are, however, of a different chemical nature since they do not take these stains, and exhibit different optical properties; and the transverse membranes of minute, dot-like elements having, again, different chemical and optical properties from the other parts. (The accessory disks, since they are inconstant, may, in this brief preliminary communication, be left out of account.) But the optical sections of the sarcostyles (figs. 6, 7a, 8, and 8a), *i.e.*, more especially of their sarcous elements, which, in teased preparations of muscles prepared by the alcohol-gold method, are frequently set free in the preparation, and are seen lying, as often as not, upon one surface, show conclusively that the above supposition regarding the fibrillar constitution of the sarcostyles is entirely erroneous. The sarcous elements are not made up of a bundle of rods, but are formed of a continuous substance (*sarcous substance*), staining with hæmatoxylin and with gold after hardening in alcohol, which substance is pierced by tubular canals which open at each end of the sarcous element, and in its middle abut against one another at the plane of Hensen's line. The optical section of each sarcous element shows a dozen or more of such canals, and the contents of these canals are, to all appearance, freely continuous with the transparent, colourless substance of the clear intervals; this can be made out in the longitudinal views. The longitudinal striation of the sarcous element is due to this canalisation; that of the clear interval to a prolongation of delicate lines (which may, perhaps, represent

thin septa) of the sarcous substance through the clear interval to the transverse membranes. The whole sarcostyle appears to be itself enclosed by a membrane of extreme delicacy.

If we assume, as is to all appearance the case, that the substance of the clear interval is of a fluid or semi-fluid nature, the above view of the constitution of the sarcostyle, which is illustrated with unmistakable clearness in the photographs, enables one to form a tolerably reasonable idea as to the physical change which may occur when the sarcostyle passes from the condition of extension to that of retraction, and *vice versâ*. For if the sarcostyle be extended, the sarcous elements being narrowed and laterally compressed by the extending force, the fluid which is contained in their canals will become squeezed out, and will pass into the clear intervals, while, at the same time, the process of extension will elongate the sarcous elements and separate them further from the transverse membranes. With further extension, a separation of the sarcous element in the middle may also occur, some of the expressed fluid passing into the interval between the two halves.*

On the other hand, when the extended sarcostyle becomes retracted (? contracted), the sarcous elements swell and the clear intervals become shorter so as eventually *almost* to disappear. This can only be effected by the absorption of the homogeneous substance of the clear intervals into the sarcous element, and in all probability it is imbibed into the canals or visible pores of the sarcous substance. The process may, in fact, be roughly compared with that which would occur with a series of pieces of sponge, placed at short intervals, in a thin-walled elastic tube filled with fluid. If the tube were extended the fluid would be squeezed out of the pores of the sponge, and would go to increase the volume of that in the intervals; on relaxing the extending force, the fluid would be re-imbibed by the sponge, and the intervals would be diminished. This comparison is not intended as an explanation of the mechanism of muscular contraction, but merely as an illustration of the physical changes which may reasonably be supposed to accompany the varying conditions of extension of the muscle.

The subject of this preliminary communication is treated of more fully in a detailed account of the structure of muscle which will shortly appear in the 'International Journal of Anatomy and Physiology.' Since, in that account, I shall have occasion to refer at

* This separation does not always occur with continued extension, for in the sarcostyle photographed in fig. 4 the sarcous elements of the extended part, although they show the effect of traction in their elongation and narrowing, are not separated and contracted in the middle, as in the sarcostyle shown in fig. 3, but are even slightly bulged at the centre. There appears, however, a slight tendency for their ends to separate as (? accessory disks).

considerable length to the views and statements of other recent writers on the same subject, and to indicate the bearings of these observations upon the wing-muscles on the more intricate subject of the structure of the leg-muscles of insects, and of the ordinary skeletal muscles of vertebrates, I have omitted such references and indications from the present notice. I may simply state, however, that for reasons which are given at length in the article above referred to, I regard the structure of the wing-muscles of insects as furnishing the key to the understanding of muscular structure in general, and I believe that it is possible to draw a comparison detail for detail between the two kinds of muscle which shows a complete correspondence in all essential particulars.

DESCRIPTION OF THE PHOTOGRAPHS. (PLATES 4 AND 5.)

All the figures upon these plates are photographs of parts of sarco-styles of the wing-muscles of the common wasp, which had been prepared and stained by the method mentioned on page 281. In specimens thus prepared there is a considerable amount of variation in the degree to which the sarco-styles, and even the sarcous elements of the same sarco-style, are swollen by the dilute formic acid, into which the muscle is placed after having been acted upon by gold chloride. This is noticeable in fig. 8, a part of which is further magnified in 8*a*, where, in the same sarco-style, some of the sarcous elements are narrow, and others wide. The latter do not, I believe, belong to contracted or retracted portions of the sarco-style, but are merely more swollen by the acid, probably because they happened to be less fixed, *i.e.*, coagulated by the previous treatment with alcohol and gold. It is noticeable also that these more swollen sarcous elements are fainter in the photographs; this is due to the fact that they are always of a bluish tint; whereas the less swollen sarcous elements are deep-red, and hence come out nearly black. The former, however, show the longitudinal striation, *i.e.*, canalisation, better than the latter. It must further be stated that the extension of the sarco-styles shown in fig. 8 has been produced in teasing the preparation with needles by the demi-desiccation process; it is quite different from the extension shown in figs. 3 and 4, which has been brought about in the living tissue prior to the advent of the hardening fluid. The sarco-styles represented in figs. 1 and 3, and the lower part of fig. 4, have been specially selected to illustrate the characteristic appearances of retraction (? contraction) and extension, because they were very distinctly red-stained and showed neither distortion from being swollen by acid nor dislocation from mechanical stretching after hardening; all the other sarco-styles which are shown in the photograph exhibit such distortion or dislocation to a greater or less extent.

PLATE 4.

- Fig. 1. Part of a sarcostyle which has become fixed in the retracted (? contracted) condition.—Fig. 1a. The same, more magnified.
- Fig. 2. Part of a retracted sarcostyle, showing a slight mechanical dislocation of some of the sarcous elements, which has been produced after hardening.—Fig. 2a. The same, more magnified.
- Fig. 3. Part of an extended sarcostyle.—Fig. 3a. The same, more magnified.
- Fig. 4. Portion of a sarcostyle, which, at one end, is much extended, at the other moderately extended, these conditions having probably been present before hardening. The middle part is somewhat dislocated, probably after hardening.—Fig. 4a. The same, more magnified.

PLATE 5.

- Fig. 5. Parts of three moderately extended sarcostyles, with granules of the sarcoplasm lying between them.
- Fig. 6. Part of two adjacent sarcostyles, somewhat swollen by the formic acid. The upper terminal sarcous element of each one is swollen and flattened out, and is lying obliquely, having been probably touched by the needle in teasing the muscle. These show, especially the right-hand one, the tubular structure of the sarcous elements.
- Fig. 7. Two sarcous elements lying free: one is represented in profile, the other in optical section.
- Fig. 8. Photograph of part of a microscopic field, containing a number of more or less broken-up sarcostyles, and showing several of the sarcous elements lying flat, and others in profile. The tubular or canalised structure is very evident. (The globules represented are oil-drops which had accidentally got into the glycerine in which the specimen was mounted.)
- Fig. 8a. Middle part of the above photograph, enlarged; *s, s*, sarcous elements in profile view. Those to which the letters are adjacent show the line where separation occurs when the sarcostyle is extended (as in figs. 3 and 3a). Some of the other (bluer) acid-swollen elements, which come out less darkly in the photograph, exhibit the canalisation better. *s', s'*, sarcous elements seen on the flat, i.e., in optical section; *o, o*, accidental oil-globules.
- Figs. 1 to 8 are photographs taken with Zeiss's 1.30 aperture, 2-mm. homogeneous achromatic objective, and with projection ocular. They are magnified 870 diameters. Figs. 1a, 2a, 3a, &c., are enlargements from the same negatives. They represent the tissue elements magnified 2300 diameters.

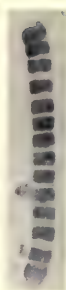
1.



1a.



3.



3a.



2, a.



2.



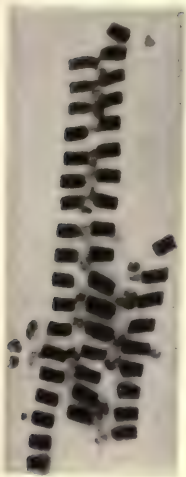
4.



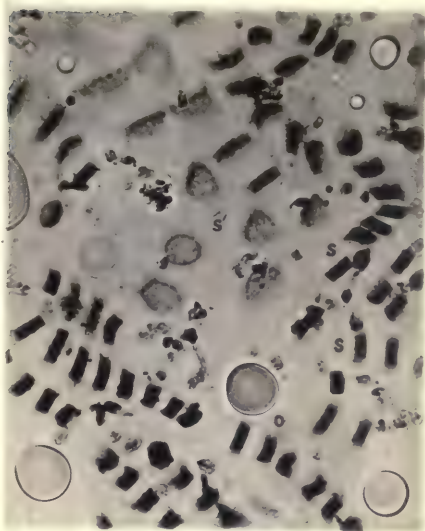
4a.







5.



6.

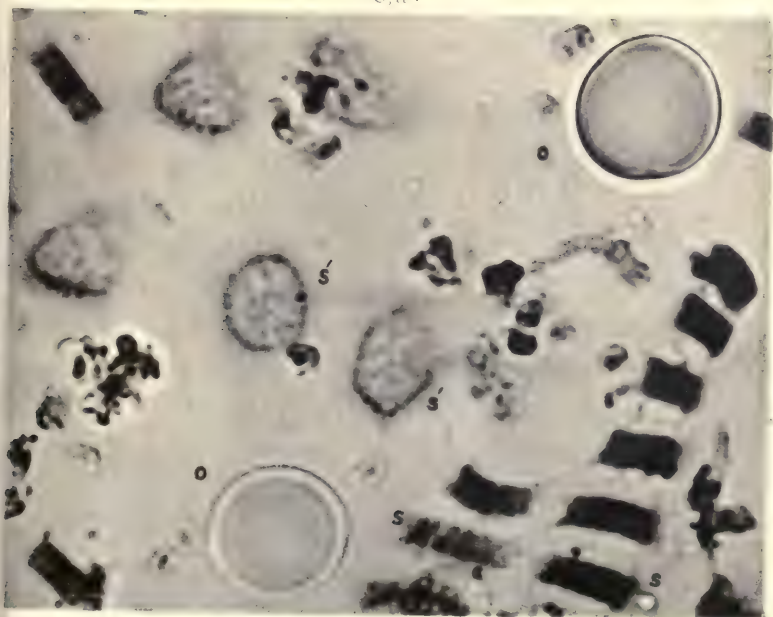


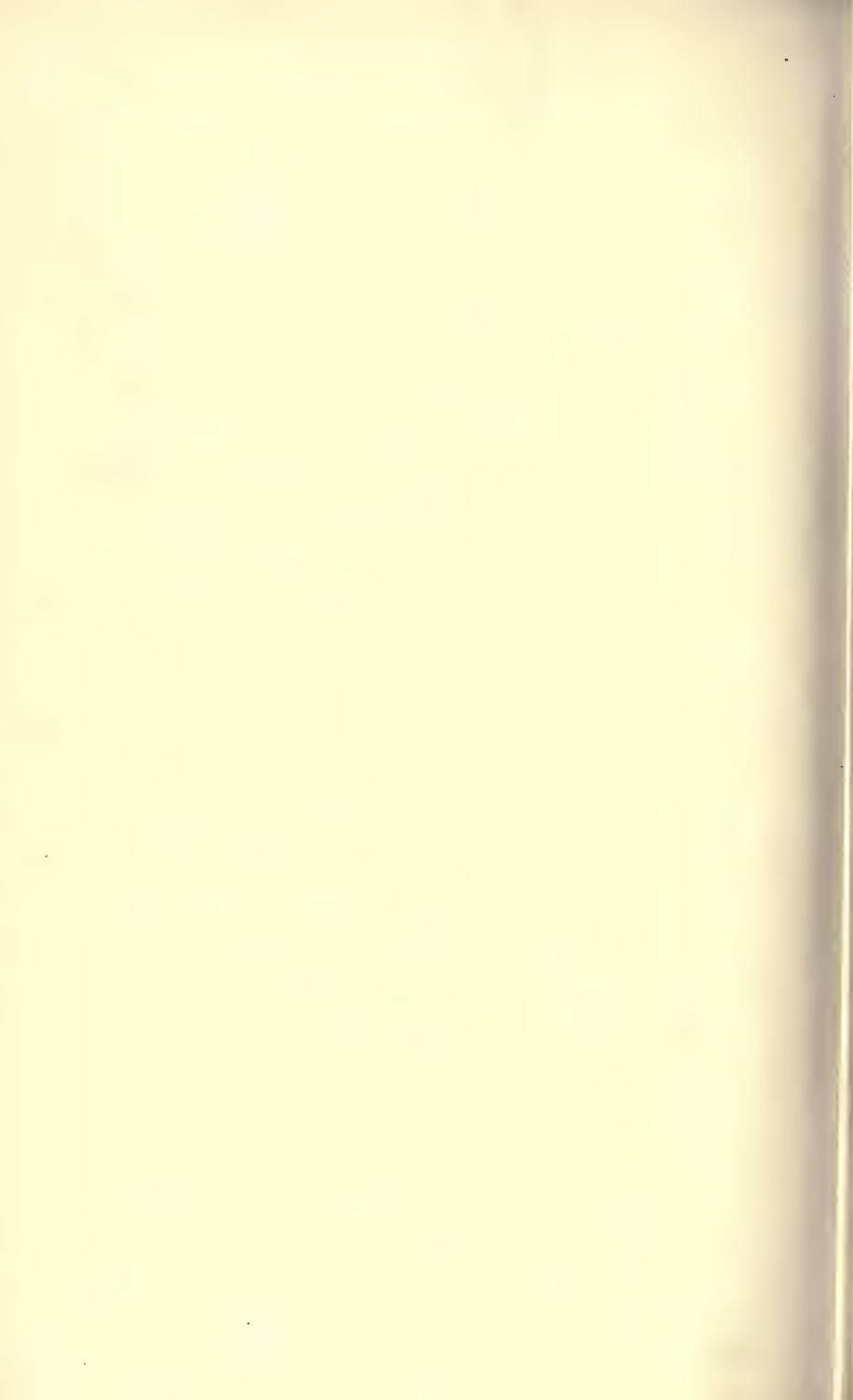
6.



7.

8, a.





“On the Minute Structure of Striped Muscle, with Special Reference to a New Method of Investigation, by means of ‘Impressions’ stamped in Collodion.” By JOHN BERRY HAYCRAFT, M.D., D.Sc., F.R.S.E. Communicated by Dr. KLEIN, F.R.S. (From the Physiological Laboratory, Univ. Edin.) Received January 2,—Read January 8, 1891.

[PLATE 6.]

Historical.

Curiously enough many of the early microscopists—Schwann for instance—recognised that the fibrils of a muscle are not simply threads of uniform thickness, like those of connective tissue: they were able to demonstrate their varicose character, even with the imperfect lenses at their command. They concluded—of course without any experimental proof—that the cross striping of the fibrils, and, therefore, of the fibres themselves, was an optical expression of such varicosity.

But Bowman, while believing apparently that the striping was optical, and comparing the muscle fibril to a beaded rod of glass, succeeded in breaking up the fibrils into little segments.

According to his view, these “sarcous elements,” as he termed them, joined end to end by cement, constituted a muscle fibril. He further believed that each sarcous element coincided in position with one of the alternating stripes, the other kind of stripe corresponding with the position of the cement joining the segments together.

But no sooner had histologists begun to associate the cross-striping with structural differences along the fibrils, than their varicosity was almost entirely lost sight of, and every new stripe (and many were discovered by Dobie, Hensen, and others) was gratuitously assumed to mark the position of some new structure.

There was, however, much to excuse what might at first sight appear to have been a great want of critical acumen, for the application of staining reagents appeared to bring out alternating differences of structure along the fibre. Thus, with logwood or picrocarmine or eosine, the clear stripe (isotropic bands), the dark stripe (Querscheibe; disque épais), the band of Hensen (Mittelscheibe; disque médian), and Dobie’s line (Querwand; strie mince), all appear to take on the stain in different degrees, so much so that, in specimens successfully prepared, some stripes appear deeply stained, others hardly at all.

Then, again, Brücke and other investigators demonstrated that the fibrils consist of alternating parts, some of which appear to be doubly, and others singly, refracting.

Overwhelmed by what appeared to be such a mass of evidence, the

histologist of ten or twenty years ago felt bound to assume that the fibril was a very complicated structure, and he never doubted that a muscular fibril consists of a series of alternating and recurring structures. It then became his duty to find out what these structures really might be, and what part they play during a muscular contraction.

The lines of Dobie are often seen as narrow dark bands, which were believed to be membranes (*Querwand*), and it was held that these membranes separated up the fibrils into little boxes (*Muskelkästchen*), so that a fibril consists, according to these authorities, of a series of little boxes, joined end to end, containing the substances whose position was marked by the other stripes. Certain of these stripes (the dim ones) were considered to mark the position of solid or relatively more solid substances, and the other stripes (the clear ones) to consist of fluid or relatively less solid substances. The appearance of the stripes, the staining, and their action on polarised light gave, at any rate, some colour to this hypothesis, for the dim stripes appear to have more substance than the clear stripes; they appear to stain with reagents, and to doubly refract light, which latter property is certainly seen in *some* solids. The light stripes, on the other hand, appear deficient in substance and solidity, they stain less readily, and they simply refract light (a property common to all liquids, and some solids).

The *Muskelkästchen* hypothesis seemed, therefore, feasible enough, and having under their microscopes little boxes containing more solid and less solid parts in alternating layers, Krause, Merkel, Engelmann, and others, sought to explain, each in his own way, the most obvious phenomenon of contractility, namely, the shortening and thickening of the contractile tissue, as being due to the interaction of these structures.

Histologists are accustomed to observe osmotic changes, the swelling up and the shrinking of red blood-corpuscles, for instance, and to see the resulting alterations of form. Under these circumstances it was not unnatural for them to suppose that during contraction the more solid parts of the *Muskelkästchen* imbibed fluid from the less solid parts, in such a way as to alter the shape of the muscle box, making it shorter and thicker, and causing, in consequence, the whole fibre to change in the same way.

In apparent support of this theory, the stripes in the muscle boxes change their relative thickness, and alter in appearance, in the manner so carefully described by these observers. There seems, in fact, only one objection to this osmotic theory which would at once present itself to the eye of the critical observer; it is the time taken by the process, for osmotic changes are slow in their very nature, and the muscles of an insect's wing can contract over a hundred times a second.

Personal Observations before the Year 1880.

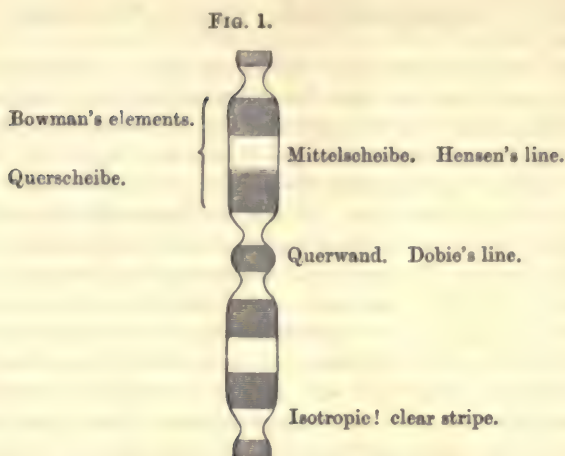
More than ten years ago it was my duty, as a young teacher, to make myself familiar with the current literature bearing upon the structure and function of muscular tissue. Even then the number of publications was very great, and I can now recall the despair with which I tried to get a grasp of a subject about which no two observers could be found to agree. While endeavouring to verify some of the statements I had read, I found out for myself that the fibrils are in reality *varicose* threads of tissue, presenting alternate swellings and constrictions of their substance. At once the conviction forced itself upon my mind that the striping might after all be an optical expression of the *form* of the fibrils, and have nothing whatever to do with their internal structure; and it was not until my results were in manuscript form, and ready for publication, that I got access to the older and almost forgotten literature in which I found the same views freely expressed, although without any attempt at their proof.

When I had made certain that, both in the fresh and in the prepared muscle, the fibrils are invariably varicose, then I felt that the position of the subject was as follows. Such fibrils are bound to be cross-striped like all other objects of similar shape, viewed by transmitted light. It may be that the cross-striping observed is due to the varicosity alone, or to the varicosity and to co-existing structural differences as well; and, under these circumstances, before we are in a position to take any further step in an investigation into the nature of the muscular fibre, it is imperative to eliminate the appearances due alone to varicosity.

My first endeavour was to ascertain whether there are any stripes that do not correspond in their position with inequalities in the thickness of the muscular fibrils.

Of course in many cases it is difficult, especially if the fibrils or fibres are somewhat distorted, to make out the border clearly; but in good specimens, in a suitable position for study, I found that the striping, both in the contracted and uncontracted fibre, corresponds invariably to either thickenings or constrictions of the fibrillar substance, and in this investigation the muscular tissue of many representative species, both Vertebrates and Invertebrates, was examined. The broad dim stripe occupies the position of a thick bulging part of the fibre, and Hensen's stripe, when present, corresponds to the position of a shallow depression in its centre. The clear stripe lies in the constrictions of the fibril and Dobie's line corresponds with a tiny swelling in its centre.

In addition, the stripes can be reversed by altering the focus, just as is the case with a little varicose glass thread, the scale of a *Lepisma*, or the shadow in the centre of a red blood-corpuscle; indeed Bowman



Part of a muscular fibril is represented in this figure, and it will be noticed that the striping of the fibril corresponds with the position of inequalities in its thickness.

actually described the striping in the reverse focus to that generally adopted, calling "clear" what we speak of as the "dim" band.

A very simple method of determining exactly what part varicosity plays in the production of the cross-striping then suggested itself to my mind; it was to immerse the fibrils in a fluid having the same refractive index. Under these circumstances it is obvious that these stripes, which are due to varicosity alone, will disappear, but the striping will become even more marked if there are alternating structures along the fibre possessed of different refractive indices. At Professor P. G. Tait's suggestion, I placed the fibres in a mixture of alcohol and oil of cassia, varying the proportions until I approximated to the refractive index of the fibrils. The striping never entirely disappeared, but it grew fainter and fainter, and I am inclined to explain the partial failure of this experiment on the grounds that, unlike a glass rod, the muscle fibres imbibed the medium in which they were embedded. As a result of this slow imbibition, the refractive index of the fibres would be constantly altering, and it would be a matter of the greatest difficulty to make it exactly the same as that of the surrounding medium. In addition to this, coagulation would be almost certain to take place within the fibrils, destroying their optical uniformity.

While looking upon the results I had obtained as valuable but not conclusive evidence, I sought to solve the problem in another way. I took the living muscle of a Crab or Fly, and, while examining it under the microscope, I pressed down the cover-glass with a

needle. Under these circumstances, those fibres which were pressed upon lost their cross-stripes, and looked extremely like connective tissue. Of course it might be urged that the fibres were by this pressure entirely disorganised, and no conclusions can legitimately be drawn from the experiment, but to this it can be replied that if there really are little bands of tissue so clearly distinguishable from each other, as those who hold the *Muskelkästchen* hypothesis believe, these or their traces should be found scattered about throughout the preparation. In point of fact, as you press upon the cover-glass the stripings gradually disappear with increased pressure, and in the ill-defined fibrillated structure that remains there are no traces of the broken *Muskelkästchen*. And finally, if more proof is wanted, it is possible by means of a screw, which raises or lowers the cover-glass, first to press upon the fibres and cause the striping to disappear, and then on raising the cover-glass to cause them to reappear once more. We can only explain this result on the assumption that the varicose fibrils are flattened out, and that the striping caused by their varicosity disappears in consequence.

There were, however, three important facts which had to be thoroughly accounted for, before it could be affirmed that the fibrils do not consist of the alternating structures supposed to exist; these facts were the effect of cleavage, of staining and the action of polarised light. The muscle fibrils can be broken across into the sarcous elements described by Bowman; but a careful study of the question soon convinced me that the cleavage *is always across the thinnest parts of the fibrils*, taking place in the substance of the clear stripe. If Dobie's line is at all marked, the cleavage takes place near the little swelling which corresponds to it, and through the substance of the clear stripe. Reference to fig. 1 will at once show that here we have to deal with the thinnest part of the fibrils, and it is therefore begging the question to assume anything over and above this mechanical reason for the cleavage, for every varicose rod will break across at its thinnest part. The phenomenon of transverse cleavage cannot therefore be taken in itself as an argument in favour of structural differences along the fibrils.

The appearances seen in stained preparations can also, I pointed out, be satisfactorily explained on the varicosity hypothesis. We find that whatever else is employed, and at whatever focus you adopt in your examination, those stripes which in the unstained preparations appear dim also appear to take on the stain, while those stripes which appear clear and bright are unaffected by it. In fact, the difference in colour is entirely a question of "saturation," for whenever there is a flood of light, as in a clear stripe, the colour of the fibre at that part becomes unsaturated by it. It is easy to convince oneself practically of this fact by the examination of varicose threads of faintly coloured

glass in a ray of parallel light. Rods of faintly tinted glass of the same shape as the muscle fibres, having tiny globules—Dobie's lines—and broader swellings for the dim stripes, when examined under the microscope, or in the field of a lantern, give as strong colour differentiations as any muscle fibres, the constrictions coming out quite colourless, while the dim band and Dobie's line are sharply brought out by their deepened colour.*

One of the chief faults of which I was guilty when publishing these results in 1880 was that I did not sufficiently lay stress on the appearances presented by a coloured or colourless varicose thread of glass when placed in the path of a parallel ray of light. It is quite different from the same thread when examined in diffuse daylight, for in the latter case a hundred images fall upon the retina at the same time, and the striping and colour differentiations are confused. One can see little appearance of striping, and if the glass is coloured it may appear very much of the same tint; place it in a lantern, or even lay it down on a piece of white paper, and the picture is quite different. As one is accustomed to view objects in diffuse daylight, one is not prepared to interpret correctly the character of such an object when viewed through a microscope: the clear well-defined bands and colour differentiations of a muscle fibril are not the appearances of a varicose thread as seen in diffuse daylight, but they are those of a similar fibre observed in parallel light when practically a single image falls upon the retina.

Lastly, we come to the action of polarised light, and here at once the phenomena by no means prove structural differentiation along the fibre. There are many questions which lead to complication. We have the varicosity of the fibril, which will alter the path of the polarised beam and produce apparent differences along the fibre when there may be in reality none at all. Then we have as a complication the interfibrillar substance, which is simply refracting, and which is chiefly lodged in the neighbourhood of the clear stripe. I was not prepared to say, under these circumstances, what is the action of polarised light on the fibrils, nor do I wish to commit myself now: it is sufficient to say that, even if we grant that alternating singly isotropous and doubly refracting anisotropic bands exist along a fibril, it does not follow that these are bands of more solid and less solid material: the whole difference may be due to molecular tension. A fibre of such a shape, as was pointed out to me both by Professor Stokes and by Professor P. G. Tait, is almost bound to possess alternating parts in different conditions of molecular tension, and give the familiar appearances when examined by polarised light.

* In doing this experiment, only faintly tinted glass must be used, and, as this is difficult to obtain, I generally use hollow varicose tubes of white glass filled with coloured fluid.

A paper containing the above results was presented to the Royal Society of London by my kind friend Professor E. Klein, and was printed in the 'Proceedings' of 1880, and in the 'Quarterly Journal of Microscopical Science,' 1881, and in this paper I ventured to assert that I had been able to explain the appearances generally considered to indicate structural differences in the course of the fibrils as being due to the varicosity of the fibrils themselves. I further stated that of course structural differences might exist, but that the proof of their existence was not as yet forthcoming.

My views were received in many quarters with kind consideration, but they were only very partially accepted. For my own part, as soon as I had published them I resolved not to think about the subject again for some years, when, with more matured experience, I might return to its consideration and picking up the threads that I had dropped unravel them with a more skilful hand.

Recent Investigations with the Collodion Impressions.

Last winter (1889-90) an idea occurred to me which led once more to my examination of the subject. It struck me that if I could "stamp" some soft transparent solid with muscle fibres it might be possible to obtain impressions of the fibres on the soft material. If these impressions had smooth unstriped depressions corresponding to the fibres, this would indicate that the striping was caused by structural differences *within* the fibrils; if, however, the impressions were striated, this could only be explained on the ground that the striation of the "stamp"—the muscle—was caused by the *form* of the fibrils, which form and which striation were transferred to the soft material in the process of stamping.

It seemed improbable that I should succeed in getting faithful impressions of such microscopic objects, yet I felt that it would be well worth while making the attempt, for the results if obtained would be most conclusive. I experimented with every substance that I could think of, using wax of various kinds, glass, gelatins, glycerin jelly, transparent soaps, &c. Once or twice I thought that I had obtained very partial success, but my difficulties were great, for whenever I hit upon a substance like gelatin, for instance, which would set in intimate contact with the fibrils, it invariably came away with them when they were removed. I worked at the subject for months, trying every expedient which suggested itself to me, and in July, 1890, I at last succeeded beyond my most sanguine anticipations.

It occurred to me that perhaps collodion might be of service, for a thin layer dries quickly and forms a beautifully smooth transparent film. I accordingly prepared a film by allowing a drop of collodion to fall upon a slide, and tilting the slide so that it flowed over it in

a layer of uniform thickness. When still somewhat moist I pressed against the film some roughly teased muscle fibres held on my finger tip. They came away quite readily when the finger was removed, leaving little "ruts" in the collodion obvious to the unaided eye. On examining these ruts with the microscope I found what I at first thought were actual muscular fibres still adhering to the collodion film, showing the fibrils and every detail of the cross striping with remarkable clearness. The ruts contained, however, no trace of muscular tissue when examined by the naked eye, for the slightest trace of muscle is at once recognised by its opacity. On looking at the specimen a few minutes afterwards, what was my surprise to find that all the appearances I had just seen had completely vanished, the ruts had disappeared, and the collodion film was flat and smooth.

The explanation was very soon found, and no doubt remained that what I had at first actually mistaken for muscular fibres were in reality their "impressions," their subsequent disappearance being due to the contraction of the film, as it dried, pulling out every inequality in its surface.

It is very instructive to watch one of these collodion impressions; at first clearly cut, with every stripe sharply defined, they gradually fade, and perhaps in five or ten minutes they disappear entirely. Sometimes a portion of a fibre really remains sticking to the collodion; it is at once recognised by its great opacity. What astonished me almost as much as the perfect reproduction in the impression of every cross stripe was the ease with which these impressions can be made. One can hardly fail to obtain them, and at the International Congress in Berlin, while demonstrating the subject to the members of the Physiological and Anatomical Sections, I made over one hundred preparations—few of which were failures. Not only can one stamp with hardened muscle, but the fresh tissue can itself be used. Of course the fresh tissue is soft and does not make so good a stamp, the results are not so striking, but they are quite evident. In making impressions of a fresh muscle one can take a piece of muscle, say from a Rabbit, cut it through in the direction of the fibres, and press the cut edge for a second or so against the collodion film, which must be very soft: one rarely examines the film without getting some trace of an impression upon it.*

If the impressions are examined with a high power, say 600 diameters, the following details can be made out. Each fibril, if a hardened

* One can get impressions of other tissues, bone, tooth, hair, &c. A section of dry bone comes out very well, and one can see in the impression the "set" of the lamellæ, the lacunæ and their canaliculæ, and every detail with marvellous clearness. If a still moist film be pressed against the back of the hand, and then examined, one sees the impressions of the imbricated scales covering the hairs on the back of the hand far clearer than in the original.

preparation be used for stamping, makes its own individual impression in the collodion, which rises between the fibrils in the place of the interfibrillar substance, which has, of course, been removed in the ordinary preparation of the tissue. When the muscle is pulled away the impressions of the individual fibres can readily be made out, and the borders of the little varicose hollows are plainly to be seen; the cross-striping, which can here only be due to the *form* of the impression, is exactly the same as that of the muscle itself. To put it in another way, we have varicose threads of air, within surrounding collodion in the place of varicose threads of muscle surrounded by balsam or Farrant, and varicosity, the only common factor in the two cases, is alone the cause of the striping observed in each. Not only are the broad stripes well marked, but one can see with even greater ease than in the muscle itself the lines of Dobie and of Hensen. In the fresh muscle I have only once or twice seen the outlines of the fibrils with any degree of distinctness, but the stripings are more readily seen; yet one would hardly expect to get such good results from fresh muscle, both on account of its softness and from the fact that the fibrils are covered by sarcolemma. If the collodion be tinted, say with magenta or Bismarck-brown, impressions can be made in this coloured medium, and these show in beautiful detail the apparent stain differentiations observed in muscle. The broad dim stripe comes out red and appears like a solid, well-defined band, and the clear stripe in successful preparations appears by contrast devoid of colour.

It will be seen from the above experiments that, as the stripings are all optical effects of varicosity, the very foundations of the Muskelkästchen hypothesis are removed, and we now come to the consideration of the phenomena of contraction.

An "*impression*" of a muscular fibre shows in every detail the appearances characteristic of the muscle used to stamp it, in whatever state of contraction or relaxation it may happen to be.—If a piece of muscular tissue, hardened in alcohol in the extended position, be examined under the microscope and its details studied, and if an impression of it be then made, the impression will show the same details that it shows. The same holds good for the contracted or semi-contracted fibre. Photograph I was very kindly taken for me by my friend Dr. Carrington Purvis, and shows the appearances presented by a Crab's muscle in a state of extension. The little varicose fibrils are seen separated by little varicose dark lines, the latter being the optical sections of the interfibrillar substance.

Photograph II* is taken from an "*impression*" of a muscle in a

* The photographs of the "*impressions*" were taken by my friend Dr. Edington, to whose skill and interest I am much indebted. As the "*impressions*" only last about five or six minutes, and as with ordinary illumination an exposure of

similar condition, and it will be noticed that the appearance is essentially the same, except that the stripes are reversed, the little dots forming Dobie's line and the dim bands coming out bright, and the clear stripe appearing dark; the slightest alteration of the focus would have reversed the photograph and have given the ordinary appearance.

A contracted fibre has quite another appearance, for not only are the cross-stripings much nearer together, but they have changed in character. Without going into detail, at present, it is sufficient to say that alternately dark and light stripes are seen, and that the Dobie's line, so constant a feature in the extended fibre, is no longer to be seen in the contracted condition; the stripes, moreover, have altered in thickness relatively to one another. Now it is needless to again point out that the change in the striping has hitherto been held to indicate changes *within* the fibrils of the nature of osmosis, the stripes being taken to represent actual structures. But if an impression be taken of a muscle killed in contraction it shows every detail of a muscle *in that condition*, as photograph III, taken from a collodion impression, very well indicates. (In this photograph the clear stripes come out clear, and the dark stripes dark, just as in the original muscle, but of course the appearance could be reversed by altering the focussing.)

It follows that when a muscle passes into a condition of contraction the changed appearance is entirely due to a change in its form, and I have frequently stamped muscles which show in the same fibre both the contracted and uncontracted state with the intermediate stages. These intermediate changes come out perfectly in the "impressions," so that one can positively affirm that the striping is due to form, and every change in striping observed during contraction depends upon some change of form too. Of course the imbibition theories of Krause, Merkel, and Engelmann are no longer tenable, since the facts on which their theories were founded have received another explanation. The Muskelkästchen was evolved on the supposition that the cross stripes correspond to membranes and layers of tissue along the fibres, whereas the impressions prove that they are due to variations in the thickness of the fibrils in different parts of their course. The imbibition theories were evolved on the supposition that the changes in the striping observed during contraction are due to alterations in

from ten to fifteen minutes is required, our first attempts were not as successful as might have been desired, and those exhibited in Berlin were decidedly faint and wanting in density. Dr. Edington subsequently, adopting a suggestion of Mr. Forgan, used magnesium light in the place of the ordinary oil lamp, burning about one foot of the thin riband in the optical axis of the apparatus. This exposure, lasting only a few seconds, gave us very beautiful negatives, from which the photographic plate was taken.

the relative quantities of fluid held by the substances producing the striping. Inasmuch, however, as the changes in the striping are due to changes in *form* of the fibrils, the very foundation of these theories has been removed.

The Author's Views as to the Structure of Striped Muscle.

Before proceeding further I would venture to state what I think we are in a position to affirm respecting the structure of striped muscle. The fibres consist of fibrils generally grouped together in bundles and separated from each other by interfibrillar matter. As the fibrils are varicose, and have a different refractive index from the interfibrillar matter in which they lie, they, in consequence, present the optical striping possessed by all such bodies under similar circumstances, and we have no reason to suppose that this striping has any other interpretation. The fibrils, from whatever point we look upon them, are composite structures, and their varicosity indicates this quite clearly. Each fibril has practically undergone segmentation into a series of tiny particles, although there is no evidence that these are separated from one another by membranes, or any other anatomical structures, and each little bit contracts on its own account so as to thicken and shorten. Although we know absolutely nothing as to what there is *within* each fibril, yet the condition of parts, whatever it may be, is probably the same in every Dobie's line, or in every dim or light stripe. Each light stripe may merely consist of contractile tissue in a different state of tension from that in the position of the dim stripes, and if so, that may partly interpret the polariscopic phenomena, but beyond the fact that a difference exists we are not in a position to make a further affirmation. When we study the change in form which these little segments undergo in passing from the relaxed into the contracted condition we come upon several curious facts, the interpretation of which is at present very difficult. Some muscles, and especially those of some of the lower Vertebrates appear to be very simple in form, and to undergo very simple changes during contraction. I hope to enter into greater detail in a subsequent paper, but in the meanwhile I would simply state that the fibrils seem to be devoid of the tiny swellings which form the line of Dobie. The fibrils, therefore, possess simply alternate swellings (dim stripes) and constrictions (clear stripes). During contraction, the swellings become more marked as the fibrils shorten, the change being represented in fig. 2.

In this case the dark stripes of the contracted fibre are at just the same parts of the fibril as in the relaxed condition. In other muscles in most of the Arthropoda, for instance, the stripes are reversed, as already so well described by the German histologists.

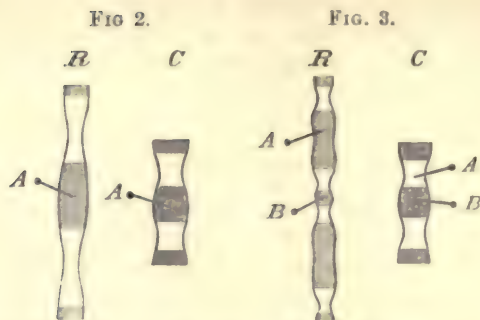


FIG. 2 (*R*) represents a relaxed fibril with a pin, *A*, sticking into the dark stripe.

During its contraction (*C*), as the fibril simply shortens and thickens without otherwise changing its shape, the needle *A* is still seen sticking in the dark stripe.

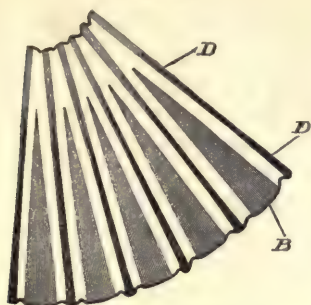
FIG. 3 (*R*) represents a relaxed fibril with a pin, *A*, sticking into the dark stripe, and another pin, *B*, sticking into the swelling in the position of Dobie's line.

When contracting (*C*), the fibril is profoundly modified in shape, the pin *A* sticking into the clear stripe, and the pin *B* into the centre of the dark stripe.

The reason is that, during contraction, the fibrils change their shape in such a manner that the parts which previously bulged now become the thinnest part (fig. 3). As the fibrils begin to contract the substance of the clear stripe becomes an eminence instead of a depression, and the little bulging part forming Dobie's lines becomes smoothed out and gradually obliterated. The dark stripe, on the other hand, becomes the constriction in the case of the contracted fibre, and, of course, appears now as a clear band. These points can only clearly be made out by studying all the intermediate conditions between complete contraction and relaxation, and they are best seen in the living muscle fibres on which waves of contraction are still slowly passing; one may see them, too, upon the muscle impressions. I have never happened to make an impression of a fibre showing a series of these intermediate stages in a short piece of a fibre while engaged with Dr. Edington in photographing them, but fig. 4 shows very well the appearance; it is a careful drawing of a Crab's muscle in a state of contraction, but bent at an angle so that the convex side is artificially extended. The Dobie's lines on the extended side are seen gradually to thin away, and gradually disappear on the contracted side, while the surrounding bands which appear as clear depressions gradually become dim elevations.

Of course, this change of form leads to the shortening of the fibrils, but it is at present difficult to say why this reversal of the varicosity should occur; at present, we have to accept it as an unexplained fact.

FIG. 4.



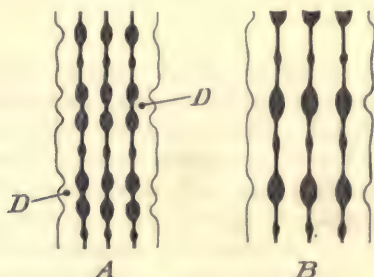
A drawing of a living and contracted Crab's muscle, which has been bent round and artificially extended on its convex lower border. The transitions between the relaxed and contracted parts are well seen. Dobie's lines (*D*) gradually fade away as you pass to the contracted condition, becoming invested by and then replaced by the dark stripe of the contracted condition. The dark stripe of the relaxed part (*B*) fades away, and is replaced by the light stripe in the contracted part.

Many of the German histologists have described a condition observable before the muscle has completely contracted (*Uebergangsstadium*), in which all striping has disappeared, and this Merkel and Engelmann each explains on his own imbibition theory. Now, one can dogmatically affirm that in the greater number of fibres of which the tissue is in one part contracted and another part relaxed, and in which the intermediate stages are plainly visible, as in fig. 4, for instance, not a trace of such a condition is visible; it is therefore not an essentially intermediate phase. I have had the privilege of seeing Professor Engelmann's preparations, and here it is seen, and in my own I occasionally come across it; but what I have invariably observed is this, that it is never seen in a fresh preparation free and unattached to cover-glass or slide. It frequently happens that fibres become pressed and otherwise fixed, and then it appears that when they shorten, the contracted part pulls upon the still extended portion, and diminishes by so doing the varicosity of the fibrils, and, in consequence, the striping which depends upon it. The effect is exactly the same as that produced by flattening out the fibres by pressing on the cover-glass; in one case the varicosity is diminished or obliterated by a pull in the length of the fibril, in the other case by pressure applied to its sides. One may, at any rate, state that in the vast majority of cases, as the varicosity becomes reversed, the fibrils never become uniform threads of tissue, for, as the dim stripe is flattening out, eventually to form a depression, the clear stripe, with Dobie's line still visible in its centre, is becoming a ridge.

The Interfibrillar Substance.

The interfibrillar substance is not usually held to have the property of contractility, and it appears to me that the arguments based on its fancied homology to a cell network recently brought forward, especially in England, can hardly be said to prove the contrary; I hope to refer to this subject in a subsequent paper, to be devoted to the comparative histology of muscle. From the varicose character of the fibrils, it follows that the interfibrillar substance is of the nature of a matrix or bed of tissue perforated by varicose tubes. It is like a honeycomb minus its transverse partitions, or, better still, like a mitrailleuse; we have, however, to imagine the walls of the honeycomb of variable thickness, sometimes thicker and sometimes thinner, and the analogy is complete. In optical section, as when we focus a piece of muscle, this interfibrillar honeycomb will appear as in photograph I or fig. 5.

FIG. 5.



Two fibres are represented, *A* and *B*. The interfibrillar substance is strongly represented by the varicose lines; the outlines of the fibrils are faintly represented at the borders of the figures. In *A* the fibrils possess well-marked Dobie's lines; the swellings of the fibrils causing masses them are seen, *D*. In consequence, the cement matter forming single masses in *B* is in *A* divided into two sets (heads of Schäfer's muscle rods). In *B* Dobie's lines are not seen. In diagram the cross-stripping¹ of the fibrils has been omitted for the sake of simplicity.

Here the fibrils are left blank, and the interfibrillar substance is represented by dark varicose lines, the optical sections of the longitudinal walls of the honeycomb. The walls are thick opposite the position of the constrictions of the fibrils which lie within the honeycomb, and thin where the bulgings come. When a Dobie's bulge is present as at *A*, the bulgings, corresponding to the clear stripe, are divided into two (Schäfer's muscle rods); but when Dobie's line is not well marked, we have the appearance seen at *B*. Of course, it will be understood that where these thickenings of the honeycomb

occur the fibres are encircled by a thicker band of inter-fibrillar substance, that the little beads or swellings in the diagrams are merely optical sections of the thicker parts of the honeycomb. These thickened portions, when very strongly differentiated from the fibrils, as by the gold method, may appear like transverse bars running across the fibrils in the region of the clear stripe, and the whole structure has unfortunately been misinterpreted by some observers into a network, the transverse links of which are the encircling and thickened parts of the honeycomb, while the longitudinal threads are the lines really marking the optical section of the honeycomb tubes. If any threads of tissue are to be actually seen, I quite agree with Professor Klein in ascribing them to precipitation within the inter-fibrillar honeycomb.

The Physiological Explanation of the Varicosity.

I may not unreasonably be asked to supply some hypothesis of my own in place of the exploded theories of imbibition, for, if we simply view a muscle fibre as consisting of varicose fibrils, we have a bare morphological fact without any physiological significance. Before doing this, I will venture to clear up one misunderstanding which has arisen concerning the morphological difference between striped and unstriped muscular tissue, although this question will, I hope, be more fully discussed in a subsequent paper.

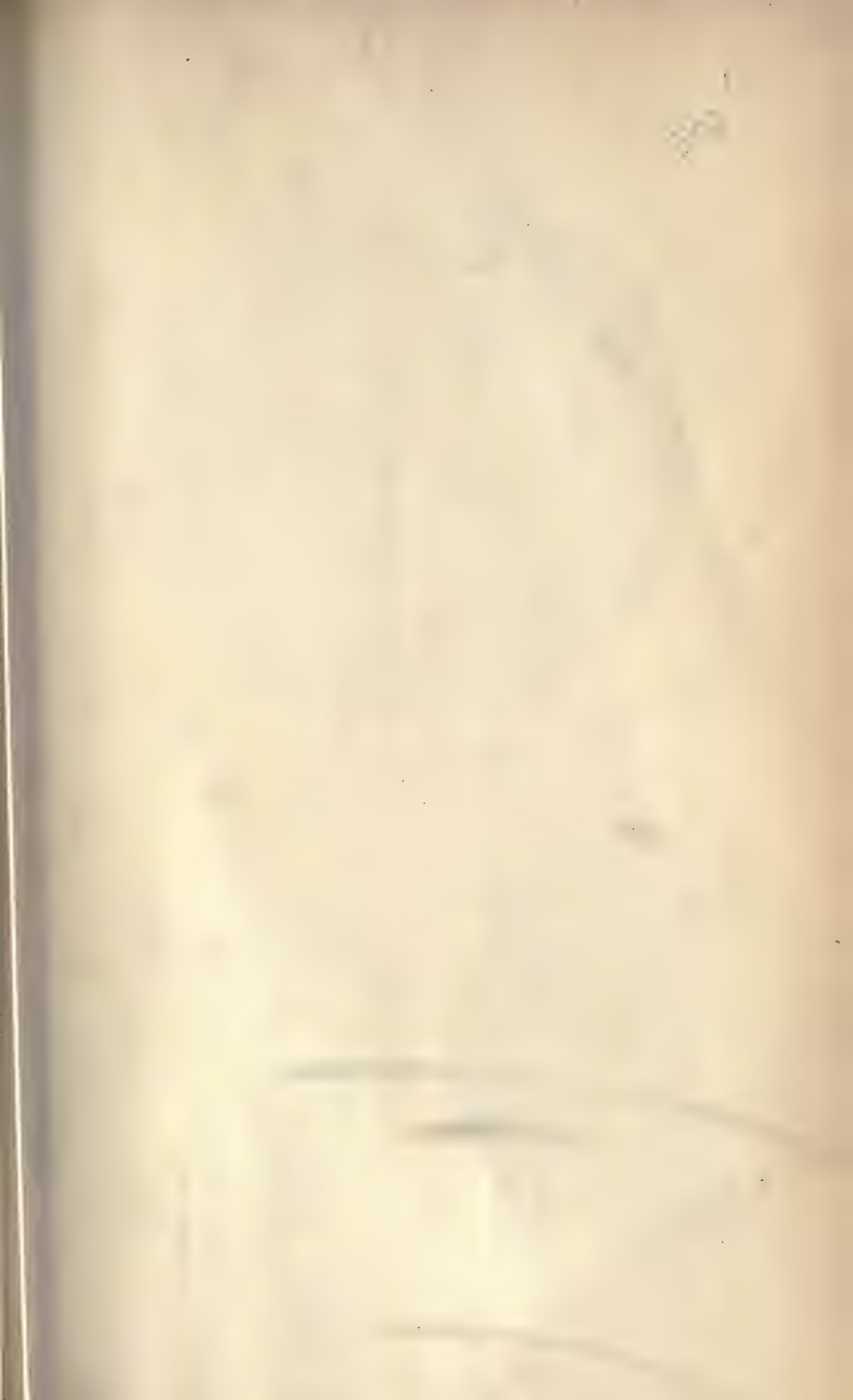
The unstriated muscle is generally described as a nucleated spindle, presenting fine longitudinal fibrillation, and devoid of a true sarcolemma, while the striped or voluntary fibre is described as a fibrillated thread of contractile tissue, invested by a sarcolemma underneath which numerous nuclei are placed. The heart muscle is generally looked upon as a tissue intermediate between the two. But authors to whom we owe these ideas, have restricted their enquiries to the Vertebrate histology alone. It is necessary to pass into the region of comparative histology, before we can thoroughly comprehend the subject. If we do this, we shall find that there are two chief varieties of fully differentiated muscular tissue. First of all, there is the nucleated spindle devoid of sarcolemma and made up of fibrils cemented together, and we notice that *these spindles may be striped or unstriped, the difference depending upon the rapidity of their contraction*. They are found in most divisions of the animal kingdom; thus, in the adductor muscles of *Cardium*, *Pecten*, *Lima*, rapidly moving Lamellibranchs, we have nucleated and striped spindles; these occur in the heart muscle of the Frog and many other animals, while non-striped spindles are found in parts of the circulating and digestive systems where less active movements are required.

Then again, there is another type of muscular tissue, consisting of

cylindrical threads, sometimes invested by a sarcolemma, and with nuclei within the fibrils, under the sarcolemma or in both of these situations, and we notice that *these threads of tissue are striped or un-striped according to the rapidity of their contraction*. In Vertebrate skeletal muscle, they contract quickly and are striped, and the same applies to the adductor of the *Terebratula* which closes its shell so quickly as sometimes to nip its protruding siphon. In many of the Polychætæ, in many Lamellibranchs, as in *Mytilus*, and in slowly moving Ascidia, the fibres are devoid of striation. We see then that the striping of muscular tissue cannot be said in any way to associate itself with any particular "build" of cell; it may be present in both a spindle and in a cylindrical thread. When a muscular fibre, it may be spindle-shaped or cylindrical in shape, is called upon in the process of evolution to contract very quickly, then it becomes striped, the cause of which is the segmentation of the previously cylindrical fibrils into varicose threads. The Swallow in its rapid flight has quickly to see, and catch the passing fly, and the fibrils of its ciliary muscle, simple threads of uniform thickness in some ancestral form, now become beaded and cross-striped.

Striated muscle may, therefore, be defined as "muscular tissues, the ultimate fibrils of which have become varicose, and this in association with the power of quicker and more active movement."

We can now ask ourselves whether it is not possible to explain this correlation between the segmentation of a muscle and its power of contracting more rapidly, and it will, I think, be seen that a very simple and straightforward explanation can at once be given. The whole subject can be resolved into a question of "mass"; the larger the contractile element is, the longer time will it take to reach its maximal degree of shortening, so that when a fibril segments into a number of much smaller particles, each one contracting and relaxing on its own account, a considerable amount of time will thereby be gained. We have many examples of the influence of bulk, or mass, upon rapidity of contraction in the case of the gross muscles themselves, the larger animals moving relatively slower than the smaller ones, as when the Hare, in spite of its smaller leaps, can nearly keep pace with the Horse, because its leaps are repeated at much shorter intervals. We can now see how, by simple means, a muscle can, during its evolution, contract more quickly, but the fundamental explanation of the phenomena of contraction is still to be found. Whether or not we may ever be able to express muscular contraction in terms of those phenomena which we see in the inorganic world I am not in a position to say, but this we must all be certain of, that this explanation will result rather from a study of the contraction phenomena of the lower and simpler types of con-



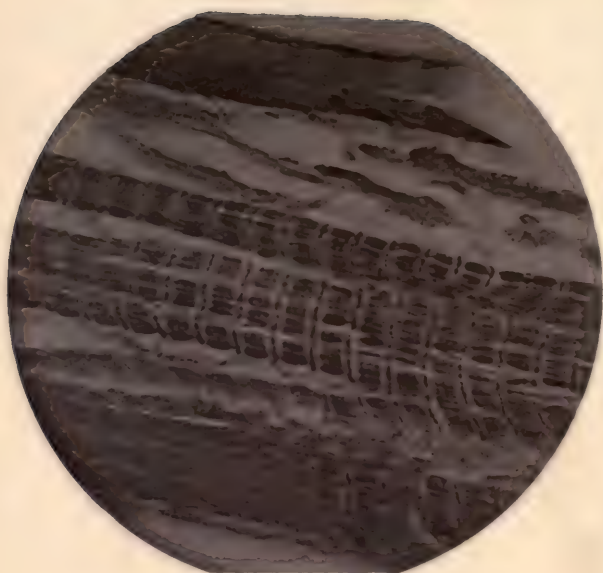


Fig. 1.



Fig. 2.

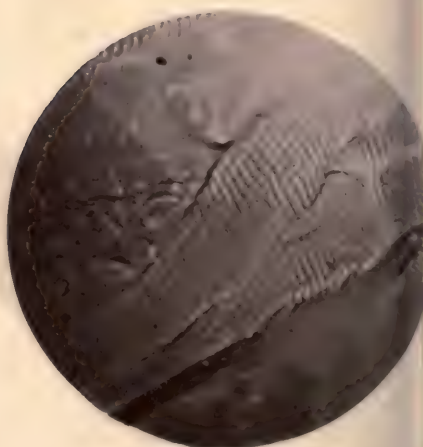


Fig. 3.

tractile tissue than from the highly evolved tissue of the striped muscle.*

EXPLANATION OF PHOTOGRAPHS. (PLATE 6.)

Photograph I.

Photograph of the muscle of a Crab in a state of relaxation, and magnified 1000 diameters. Dobie's lines are seen as narrow dark bands running across the fibres, and corresponding to tiny bulgings of the individual fibrils; seen best at the upper edge of the fibre. The clear stripes on either side of Dobie's lines correspond with constrictions in the fibrils, and the dark stripes correspond with broad swellings. The cement substance between the fibrils appears light in colour.

Photograph II.

Photograph (700 amplifications) of a moist film of collodion, upon which a piece of relaxed Crab's muscle had been pressed and had then been withdrawn. In this "intaglio" all the appearances of the relaxed Crab's muscle are to be seen, those parts which are dark in Photograph I coming out white in the intaglio. The cement matter and the clear stripes are dark, and the dark stripes and Dobie's lines come out light in colour.

Photograph III.

Photograph of a moist film of collodion, upon which a piece of contracted Crab's muscle had been pressed and had then been withdrawn. The striping is that of the contracted fibre in all its detail; the approximation of the cross-stripes to each other and the absence of Dobie's line are points especially to be noted. Owing to the collodion film varying in its thickness, the intaglio is photographed at different focal planes, and the dark stripe, which appears light in the lower part of the photograph, comes out dark in colour at the upper part. The edge of the intaglio is better seen than in Photograph II, and by the aid of a lens one can readily see in the original negative the interfibrillar matter.

I am much indebted to the Cambridge Engraving Company for the excellent manner in which the photographs just described have been reproduced.

* Professor P. G. Tait has recently suggested to me, that, owing to their varicosity, the fibrils will be able, as it were, to get a better "grip" of the interfibrillar matter, so that during contraction or relaxation the muscle will be able more effectually to move as a whole.

March 5, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of the Candidates for election into the Society were read from the Chair, as follows:—

Anderson, William, M.Inst.C.E.	Gilchrist, Percy C.
Bateson, William, M.A.	Gotch, Francis, M.R.C.S.
Beddard, Frank Evers, M.A.	Halliburton, William Dobinson, M.D.
Beevor, Charles Edward, M.D.	Harcourt, Professor Leveson Francis Vernon, M.Inst.C.E.
Blake, Rev. John Frederick, F.G.S.	Heath, Christopher, F.R.C.S.
Boulenger, George Albert.	Heaviside, Oliver.
Bower, Professor Frederick Orpen, D.Sc.	Herdman, Professor William Abbott, D.Sc.
Buzzard, Thomas, M.D.	Hickson, Sydney John, D.Sc.
Cheyne, Professor William Watson, F.R.C.S.	Howorth, Henry Hoyle.
Conroy, Sir John, Bart., M.A.	Joly, John, M.A.
Crisp, Frank, LL.B.	Jones, Professor John Viriamu, M.A.
Cunningham, Professor Daniel John, M.D.	Kidston, Robert, F.G.S.
Davis, James William, F.G.S.	King, George.
Dawson, George Mercer, D.Sc.	Lansdell, Rev. Henry, D.D.
Dibdin, William J., F.C.S.	Larmor, Joseph, D.Sc.
Dickinson, William Howship, M.D.	Lydekker, Richard, B.A.
Dreschfeld, Professor Julius, M.D.	Macalister, Donald, M.D.
Eaton, Rev. Alfred Edwin, M.A.	McConnell, James Frederick Parry, Surgeon - Major, F.R.C.P.
Edgeworth, Professor Francis Ysidro, M.A.	MacMunn, Charles, M.D.
Elliott, Edwin Bailey, M.A.	Marr, John Edward, M.A.
Ellis, William, F.R.A.S.	Matthey, Edward, F.C.S.
Foster, Professor Clement Le Neve, D.Sc.	Mond, Ludwig, F.C.S.
Frankland, Professor Percy Faraday, B.Sc.	Newton, Edwin Tully, F.G.S.
Gadow, Hans, M.A.	Nicholson, Professor Henry Alleyn, M.D.
	Ord, William Miller, M.D.

Pedler, Professor Alexander,
F.C.S.

Reade, Thomas Mellard, F.G.S.

Roberts, Ralph A., M.A.

Rutley, Frank, F.G.S.

Seeböhm, Henry, F.L.S.

Shaw, William Napier, M.A.

Sherrington, Charles Scott,
M.B.

Stebbing, Rev. Thomas Roscoe
Rede, M.A.

Stevenson, Thomas, M.D.

Stewart, John Heron Maxwell

Shaw, Major-General R.E.

Thompson, Professor Silvanus
Phillips, D.Sc.

Thomson, Professor John Millar,
F.C.S.

Thornycroft, John Isaac, M. Inst.
C.E.

Tizard, Thomas Henry, Staff-
Commander R.N.

Tuke, Daniel Hack, M.D.

Veley, Victor Hubert, M.A.

Waller, Augustus D., M.D.

Woodward, Horace Bolingbroke,
F.G.S.

Young, Professor Sydney, D.Sc.

The following Papers were read:—

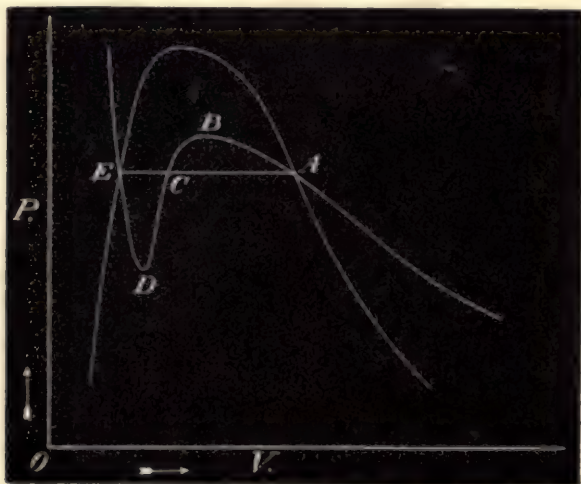
- I. "Some Suggestions regarding Solutions." By WILLIAM RAMSAY, Ph.D., F.R.S., Professor of Chemistry in University College, London. Received February 16, 1891.

The brilliant presidential address of Professor Orme Masson at the Chemical Section of the Australasian Association for the Advancement of Science marks a distinct advance in our ideas of solution. The analogy between the behaviour of a liquid and its vapour in presence of each other and of a pair of solvents capable of mutual solution is so striking as to carry conviction. The resemblance of the liquid-vapour curve, with its apex at the critical point, to the solubility curve, with its apex at the critical solution point, appears to me to prove beyond cavil that the two phenomena are essentially of the same nature. The address will take rank along with van't Hoff's classical paper on "Osmotic Pressure."

There are two other phenomena, which, it appears to me, are made clear by the ideas of Professor Masson. The first of these has reference to supersaturated solutions. The curves (published in 'Nature,' vol. 43, p. 348, Feb. 12, 1891) showing the analogy between liquid-gas and solution curves, are isobaric curves, or, more correctly, they represent the terminations of isobaric curves in the region of mixtures, where, on the one hand, a liquid exists in presence of its vapour, and, on the other, one solvent in presence of another (for both solvents play the part of dissolved substances as well as of solvents). M. Alexéeff's data are not sufficient to permit of the construction of a curve representing a similar region mapped out by the termination of isothermal lines. But it is obvious that it

would be possible to determine osmotic pressures of various mixtures by the freezing-point method, and so to construct isothermal curves for such mixtures of solvents. And there can be no reasonable doubt that, as the isobaric curves of liquid-gas and of solvent-solvent display so close an analogy, the isothermal curves would also closely resemble each other.

Granting then that this is the case, we may construct an imaginary isothermal curve on the model of the curve for alcohol published in the 'Phil. Trans.' by Dr. Sydney Young and myself. Now, in one series of papers on the liquid-gas relations, we showed that with constant volume pressure is a linear function of temperature; and we were thus able to calculate approximately the pressures and volumes for any isothermal representing the continuous transition from the gaseous to the liquid state (see 'Phil. Mag.,' 1887, vol. 23, p. 435). It would be interesting to ascertain whether, if concentration be kept constant, osmotic pressure would also show itself to be a linear function of temperature. But, this apart, it appears in the highest degree probable that there should also exist, in theory, at least, a continuous transition from solvent to solvent, the representation of which would be a continuous curve. In such a case, on increasing the concentration of the solution by eliminating one solvent, the other solvent should not separate visibly, but the two should remain mixed, until one solvent has been entirely removed.



The accompanying diagram will make this clear. The sinuous curve ABCDE may represent either continuous change from gas to liquid along an isothermal on decrease of volume, or it may

represent a similar continuous change from saturated solution to dissolved substance on increase of concentration.

Mr. Aitken's experiments on the cooling of air containing water-vapour have shown us that it is possible to realise a portion of the curve AB; the phenomenon of "boiling with bumping" constitutes a practical realisation of a portion of the curve DE; and we may profitably inquire what conditions determine such unstable states with solvent and solvent.

Regarding the portion of the curve AB, I think that no reasonable doubt can be entertained. It precisely corresponds to the condition of supersaturation. In the liquid-gas curve, the volume is decreased at constant temperature without separation of liquid; in the solvent-solvent curve the concentration is increased without separation of the solvents. Dr. Nicol has shown that it is possible to dissolve dry sodium sulphate in a saturated solution of sodium sulphate to a very considerable extent without inducing crystallisation; and here we have a realisation of the unstable portion of the curve AB. In the gas-liquid curve pressure falls with formation of a shower of drops; in the solvent-solvent curve crystallisation ensues, and the solvents separate. The phenomena are, however, not completely analogous; the complete analogy would be if the temperature were so low that the substance in the liquid-gas couple were to separate in the solid, not in the liquid, state. This, so far as I am aware, has not been experimentally realised, but one sees no reason why it should not be possible.

I have some hesitation in offering speculations as to the state of matter at the portion of the continuous curve DE. It may be that it corresponds to a syrupy or viscous state. Cane-sugar at a moderate temperature dissolves water; indeed it is possible to obtain a solution of 1 per cent. of water in molten cane-sugar. And such a solution, if quickly cooled, remains a syrup. But it can be induced to crystallise by the presence of crystals. Thus, in such a mixture of sugar and water, a few grains of crystalline sugar cause the whole mass to crystallise, and water saturated with sugar and sugar separate into two layers. Here, again, a complete analogy fails us, for it is a solid which separates. As we know nothing of the osmotic pressure of a syrup, the analogy is a defective one; but it is probable that a dilute solution of sugar would pass continuously into a syrup of pure sugar by evaporation of the solvent, and analogy would lead to the supposition that the syrup coincides with the unstable state of the liquid. I would, therefore, offer the analogy between the syrupy and the supercooled states as a tentative one; it lacks foundation in both cases.

One point remains to be mentioned. I have for the past nine months, in conjunction with Mr. Edgar Perman, been determining

the adiabatic relations for liquid and gaseous ether: the rise of pressure and temperature when volume is decreased without escape of heat. It is obvious that similar relations are determinable for solutions, and probably with much greater facility. M. Alexéeff has made some measurements which might be utilised for this purpose; but they are far too few in number, and, moreover, the necessary data as regards osmotic pressure are wholly wanting. It would be possible by a series of differential experiments to ascertain the evolution of heat on increasing concentration, and so to arrive at a knowledge of the specific heats of the solution at constant osmotic pressure, corresponding to the idea of specific heat at constant pressure; and also of specific heats at constant concentration, corresponding to specific heats at constant volume. I do not know whether such researches would yield as accurate results as those we are at present carrying out, but they are at least well worthy of attention.

II. "Preliminary Notice of a New Form of Excretory Organs in an Oligochætous Annelid." By FRANK E. BEDDARD, M.A., Prosector of the Zoological Society. Communicated by Professor E. RAY LANKESTER, M.A., LL.D., F.R.S. Received February 19, 1891.

So far as our knowledge of the Oligochæta goes at present, the excretory system appears to consist either of one or more pairs of separate nephridia in each segment, or of a diffuse, irregularly arranged system of tubules with numerous external pores upon each segment, and often with numerous cœlomic funnels in each segment; there may or may not be a connexion between the tubes of successive segments. All the aquatic Oligochæta have nephridia of the first kind; a large number of the terrestrial Oligochæta have nephridia of the second kind; there is occasionally in the latter forms a specialisation of part of the diffuse nephridial system into a pair of large nephridia; these species connect the two extremes. But in all these Worms the nephridia are contained in the cœlom, though some of the connecting branches may be retroperitoneal; the ducts which lead to the exterior may branch in the thickness of the body wall, but there does not seem to be any extensive ramification and anastomosis of the tubes in the muscular layers of the body wall.*

I have recently found a remarkably different arrangement of the nephridia in an Annelid belonging to a new genus of Eudrilidæ. This family is chiefly noteworthy on account of the remarkable modi-

* 'Quart. Journ. Micr. Sci.,' vol. 28, Pl. xxx, fig. 1, n, and fig. 2.

fications of the reproductive organs, and the present genus is no exception to the rule in that particular; but it shows a further peculiarity in the structure of the nephridia; the arrangement of these organs in the clitellar region of the body is unique among Annelids, and is to a certain extent suggestive of the condition of the organs supposed to be nephridia in certain Nematoidea. Throughout the body generally, as in other Eudrilids, the nephridia are paired; in the genital region I was struck, on dissecting the worms, by the apparent absence of nephridia. Sections through the body wall in this region show that the longitudinal and transverse muscular layers are traversed by a system of peculiar canals not at all like nephridia in appearance. These canals are not mere clefts between the muscular fibres, such as Kükenthal has described in his paper "Ueber die lymphoiden Zellen der Anneliden;"* such lymph spaces I have found in a good many Oligochaeta, but they never possess a definite wall. On the contrary, the canals which I describe here have a definite darkly-staining wall, with nuclei here and there. They resemble the blood vessels very closely, and might easily be confounded with them.

These vessels are arranged in a longitudinal and a transverse series with numerous branches and interconnexions. The longitudinal muscles are imbedded in a nearly homogeneous, transparent, connective tissue, which is of some thickness between the peritoneal epithelium and where the muscular fibres end. It is in the latter tract of tissue that the four principal longitudinal trunks run, corresponding in position to a line connecting the four successive pairs of setæ; there appear to be smaller longitudinal trunks, but the four principal ones run through several segments without a break; these longitudinal trunks are connected with a metamerically repeated system of transverse vessels; these lie between the transverse and longitudinal muscular coats, and appear to run right round the body. They are of considerable calibre, but not so wide as the longitudinal trunks; I could not detect any ciliation anywhere, and their walls are extremely thin. They give off numerous branches, which traverse the body wall in every direction, and form a finer meshwork of tubules; some of the branches run towards the epidermis, and although I could not detect in transverse sections the actual orifices, on account of the fineness of the tubes, I could make out at frequent points a slight modification of the epidermis which seemed to correspond to an external pore.

Upon fragments of the chitinous cuticle being stripped off and examined with a high magnifying power, the orifices were quite plain. They were much smaller than the nephridiopores of *Perichaeta*, but not so minute as to be confounded with the pores of the gland cells of the epidermis.

* 'Jenaische Zeitschr. f. Naturw.,' vol. 18 (1885), p. 319.

The system of tubes was everywhere accompanied by blood vessels ; but, it is perhaps unnecessary to remark, there was nowhere any connexion between these tubes and the capillaries ; no coagulated blood was in a single instance found in the excretory tubules.

In spite of their very different appearance, as well as arrangement, from the nephridia of other types, such as *Perichæta*, which possess a diffuse nephridial system, the excretory nature of these tubes seems probable, without any further description. A connexion with the body cavity must be proved in order to remove all doubts as to their nature ; in each segment, just behind the pair of setæ, the longitudinal duct gives off a branch, which passes through the peritoneum and comes to lie in the cœlom ; this branch continues for a short distance, and then abruptly ceases ; whether it is furnished with an actual orifice or not I am unable to say. In a few cases, the branch entering the cœlom became connected with a very small coiled nephridial tubule, so small that it was not, as already mentioned, recognisable in dissection.

I am inclined to refer the atrophy of the intra-cœlomic part of the nephridia to their having been used up in the formation of the genital ducts. I have recently communicated to this Society a notice of the development of the genital ducts out of nephridia in *Acanthodrilus* ;* and that mode of development is possibly general. In any case the nephridial system of the genital segments of this *Eudrilid* consists almost entirely of a complex system of tubes, which ramify in the thickness of the body wall, which open by numerous pores on to the exterior, and are connected by a few short tubes with the body cavity. If the tubes leading to the cœlom became obliterated, and they are very short as it is, the excretory system would consist only of the network in the body walls.

This system of tubes in the skin may perhaps be more comparable to the nephridial network of Cestodes and other flat Worms, than the intracœlomic network of other Oligochæta ; its presence, however, in the body walls suggests a comparison with the Nematodea, which appear to possess at least the remains of a cœlom. In some of these Worms a system of fine tubes connected with the excretory pore permeates the interspaces between the longitudinal muscles. In *Echinorhynchus* the tubes connected with the lemnisci also ramify in the integument, and the lemnisci themselves are processes of the body wall depending into the cœlom.

* ' Roy. Soc. Proc.,' vol. 48, 1891, p. 452.

III. "Contributions to the Study of the Connexion between Chemical Constitution and Physiological Action. Part II."
 By T. LAUDER BRUNTON, M.D., F.R.S., and J. THEODORE CASH, M.D., F.R.S. Received March 2, 1891.

(Abstract.)

In a former paper, the authors discussed the alterations which are produced in the action of ammonia by the substitution of alkyl radicals for hydrogen, and by combination of the compound ammonias with different acid radicals.

In the present paper, they have examined on a similar plan the physiological action of some bodies of the aromatic series.

The research was begun more than four years ago, a preliminary communication having been made to this Society on March 24th, 1887. A good deal of work has been done in connexion with the subject by other observers while the research was in progress. The results obtained by others, however, are not easy of comparison, while the experiments of the authors, having been made as nearly as possible under the same conditions, yield results which are more easily compared, so as to allow of general conclusions being drawn from them. They have examined (1) the physiological action of benzene, and (2) the alterations which occur in its action when one or more atoms of hydrogen in it are replaced by (*a*) haloid radicals, (*b*) alcohol radicals, (*c*) by hydroxyl, (*d*) by NO_2 , and (*e*) by amidogen, NH_2 .

They have also examined the modifications in the action of various members of the series by changes in temperature.

They describe the general symptoms produced by benzene and its compounds in frogs and rats, their action on muscle and nerve, on reflex action, on respiration, and circulation.

They describe a new method of registering the blood pressure and pulse, using a slow drum for the former, and a quick one for the latter, so as to have the whole course of the blood pressure during an experiment given in a comparatively short tracing, while samples of the pulse waves are taken at various periods.

They find that the action of benzene and its compounds is chiefly exerted on the spinal cord, although they act also on the cerebrum and, to a slight extent, on nerves and muscle. Their effect on muscle and nerve is to weaken them, the paralysing action being stronger upon the nerve than the muscle.

Their action on the cerebrum is evidenced by lethargy and disinclination to voluntary movement both in frogs and rats.

Their action on the spinal cord appears to consist in producing

increased excitability, greater diffusion of stimuli with diminished power and definiteness of movement. Thus slight stimuli in the frog produce movement more readily in the poisoned than in the normal condition, but the movement, instead of being limited to one limb, vigorous and steady, is diffused over several limbs, feeble and tremulous or jerking.

In frogs, the tremors or jerking always occur on attempted movement, and sometimes, to a slight extent, when at rest. If the dose be large, they are succeeded by paralysis. Absorption of the drug is slow and irregular, and it may cause local rigor of the muscles. The heart remains long irritable.

Haloid radicals do not modify the action of benzene to the same extent as they do that of ammonia, but they do so in somewhat the same direction as the authors described in their former paper on this subject. Monochlorobenzene affects the spinal cord more than benzene, causing spasm and rapid diminution of reflex. It also weakens the circulation, but does not seem to affect motor nerves or muscles more than benzene. The bromo- and iodo-compounds have a more powerful paralysing action on the cerebrum than benzene and chlorobenzene, and the compound of iodine with benzene, like its compound with ammonia, appears to have a special tendency to paralyse motor nerves, muscles, and cerebral reflexes, and to depress the heart. Heat accelerated and cold retarded the action of the substances.

The substitution of alcohol radicals for hydrogen in benzene appears to modify its action in much the same way as one would expect from a general consideration of the properties of the alcohol group, which, as a rule, have a sedative action on the nervous system.

The compounds of benzene with alcohol radicals produce less tremor, less hyperæsthesia, and greater lethargy than the halogen compounds. The circulation is little affected by them. They have little action on muscle or nerve, but act more powerfully on the nerve than on the muscle. Their action appears to be more fleeting than that of the halogen compounds. Trimethylbenzene (mesitylene) was more active than methyl- or dimethyl-benzene. In poisoning by dimethylbenzene a curious increase of reflex action was observed, after it had almost gone, and spontaneous movement had quite gone.

Substitution of hydrogen by hydroxyl increases the tendency to convulsions. These are due to the action of the substances on the spinal cord and not on the cerebrum; they occur independently of voluntary movement, except when the dose is very small, and continue almost unchanged after destruction of the cerebrum. Slight tremor may occur before destruction of the cerebrum, but it is greatly masked by the powerful contractions referred to. The position of the

hydroxyl groups in the di- and tri-oxybenzenes affects their physiological action. Para-oxybenzene (resorcin) has an action similar in kind, but weaker than the ortho- and meta-oxybenzenes (hydroquinone and pyrocatechin). The most characteristic feature of its action is the occurrence, at nearly regular intervals, of clonic convulsions, which never become tonic or tetanic, and are due to the action of the drug on the cord. They are abolished by the action of curare, even in a limb protected by ligature from the action of both poisons. Strychnine produces tetanic spasm in a frog poisoned by resorcin, if the symptoms due to the latter drug are only imperfectly developed, but does not do so if the clonic spasms have become well marked. Large doses cause paralysis, destroying the irritability and conducting power of the cord. Trioxybenzene (1 : 2 : 3-pyrogallol) produces more lethargy than resorcin, less tremor on movement, and little spontaneous jerking. Its power to produce immediate symptoms in the frog is only one-fourth or one-fifth that of resorcin, but it is almost exactly equal to it in its ultimate lethal power.

Amidobenzene (anilin) may be regarded either as benzene with one hydrogen replaced by amidogen, NH_2 , or as ammonia in which one hydrogen is replaced by phenyl, C_6H_5 . In conformity with this constitution, the symptoms produced by it differ from those of benzene and resemble those of ammonia in the tendency to more violent spasm and to greater paralysis of muscle and nerve. They differ from those of ammonia in the fact that the convulsions never assume the form of true tetanus, the tetanic spasm which the ammonia group would produce being broken up, so to speak, by the action of the phenyl. With the exception of the hydroxyl compounds, amidobenzene causes the most rapid occurrence of motor phenomena. It produces great tremor after a spring and active incoordinate movement, but no tonic spasm. Nitrobenzene causes lethargy with increasing tremor on movement, and early abolition of reflex action.

The effect of several benzene compounds on reflex time was observed. The oxybenzenes could not be tested on account of the spontaneous jerks to which they give rise. The general action is to cause a lengthening in the reflex time, but a primary shortening was observed frequently in the case of chlorobenzene, slightly in methyl-, dimethyl-, and ethyl-benzene.

In producing muscular rigor, chlorobenzene is considerably more powerful than the bromo- or iodo-compound, and is intermediate in strength between methyl- and dimethyl-benzene. Of the methyl-benzenes, the methyl- is the strongest, the dimethyl- next, and the trimethyl- weakest. The action of these compounds on muscles is, therefore, inversely to the amount of methyl substituted for hydrogen in the benzene molecule. Ethyl benzene is nearly the same strength as methyl, and stronger than the dimethyl or trimethyl com-

pounds. Amidobenzene and nitrobenzene are less active in producing rigor.

The respiration is considerably and early affected in warm-blooded animals (cats) by benzene and its compounds. There is usually a primary acceleration, followed by slowing. The heart appeared to stop before the respiration in poisoning by benzene and its haloid compounds, by ethylbenzene, amidobenzene, and nitrobenzene, whilst respiration usually failed before the heart, or nearly at the same time, in poisoning by the methylbenzenes and oxybenzenes.

The first effect of the benzene compounds on the pulse or on blood pressure is usually a quickening of the pulse and a rise in the pressure. This is followed by slowing of the pulse and fall of the pressure.

In their preliminary communication in 1887, the authors directed attention to the curious resemblance between the tremor-caused by benzene and some other aromatic substances in frogs and the symptoms of disseminated sclerosis in man. In the present paper, they point out also the likeness between the violent slapping movements caused in the frog by some of the haloid compounds of benzene, as well as by amidobenzene, and the symptoms of locomotor ataxy in man.

IV. "The Physiological Action of the Paraffinic Nitrites considered in connexion with their Chemical Constitution. Part I. The Action of the Paraffinic Nitrites on Blood Pressure." By J. THEODORE CASH, M.D., F.R.S., Professor of Materia Medica in the University of Aberdeen, and WYNDHAM R. DUNSTAN, M.A., Professor of Chemistry to the Pharmaceutical Society of Great Britain. Received March 4, 1891.

(Abstract.)

CONTENTS.

- I. Introductory.
- II. Description of the Nitrites and of the processes used in preparing them.
- III. Action of Amyl Nitrite. Description of the Method of Investigation.
- IV. Action of other Paraffinic Nitrites contrasted with that of Amyl Nitrite.
- V. General Summary of Blood Pressure Experiments.
- VI. General Consideration of the Modification of Nitrite-action induced by Splanchnic Stimulation and Section.
- VII. Action of Nitrites on the Human Subject.

The present investigation was commenced three years ago, in order to throw further light on the mode of action of the paraffinic nitrites when introduced into the animal organism, and particularly to deter-

mine in what manner this action is conditioned by the different chemical constitution of the various nitrites employed. Since the chemical constitution of these compounds is well established, and their molecules are comparatively simple in structure, and, moreover, as their principal physiological effects are capable of accurate quantitative study, it seemed likely that the inquiry would furnish valuable pharmacological results.

Our knowledge of the physiological behaviour of the organic nitrites has been almost wholly derived from the study of amyl nitrite, which has been observed to produce a similar but far greater effect than its lower homologue ethyl nitrite, whose action, however, has not hitherto been so closely examined as that of the amyl compound. Unfortunately it seems certain that the results which have been obtained with amyl nitrite are to a large extent vitiated by the circumstance that, as a rule, insufficient pains have been taken to procure the nitrite in a chemically pure state, whilst, in addition, the usual mode of administration has been such that it is impossible to determine exactly how much of the compound has actually been inhaled.

It is believed that both these sources of error have been obviated in the present research. The exact composition of each substance was known, and a special apparatus was devised for ensuring the inhalation without loss of a definite amount of nitrite, through the trachea in animals, and through the nostrils in the human subject.

In this, the first part of the communication, an account is given of the principal work which has already been done on this subject, and this is followed by a brief description of the method by which the nitrites have been prepared and their purity ascertained. The physiological actions which have been made the subject of special study are those on blood pressure, pulse, and respiration, whilst the action on striated muscular fibre has also been fully examined. The present paper deals almost entirely with the action of various nitrites on blood pressure, and with the special apparatus used in studying it. A subsequent paper will have reference to the action of these same nitrites in producing contraction of striated muscle, and will conclude with a discussion of the whole of our results, both in their chemical and physiological aspects.

The nitrites have been prepared by the reaction of the corresponding alcohol, previously purified, with sodium nitrite in the presence of dilute acid. This has proved to constitute a satisfactory plan of preparing the entire series of nitrites with which we have worked. The liquid nitrites, after having been thoroughly washed and dried, were repeatedly distilled, in some cases under reduced pressure, until a liquid boiling at a constant temperature was obtained. Proof that the liquids thus obtained had the composition of the required nitrites

was furnished by analysis. The nitrites which we have prepared are those of methyl, ethyl, primary propyl, secondary propyl, primary butyl, secondary butyl, tertiary butyl, isobutyl, α -amyl, β -amyl, and tertiary amyl. Certain of these nitrites were prepared by us for the first time, while of those which had already been described some have been found to possess different physical properties to those usually ascribed to them. For the purposes of administration, a known volume of each nitrite was taken. The relative density of each substance having been previously determined, the weight corresponding to the volume taken was readily calculated, and from this was ascertained the amount of the active nitrite group (NO_2) present.

The apparatus for recording alterations in blood pressure consisted of a mercurial manometer writing upon a slowly rotating drum, and a Fick's kymograph writing upon a more rapidly revolving Balzac's cylinder. These manometers could be employed together or separately, but, as a rule, when pressure and number of pulsations only were being observed, both were kept open. The advantage of the arrangement is that a considerable period of time is represented by a short lineal movement on a small drum, whilst on the quick one the pulse can be reckoned and the course of the rapidly occurring variations of pressure studied. Respiration was recorded on a registering Marey's tambour attached to a double tambour placed on the thorax of the animal. An electrical signal, in connexion with a key and Daniell's cell, was placed beneath the point recording the blood pressure in order to mark the time of administration of nitrite. In cases where vagus, splanchnic, or sciatic stimulation was employed, a double key admitted the faradic current from the secondary coil of a du Bois-Reymond's inductorium to the electrodes on which the nerve rested, while at the same time it closed the signal circuit indicating the length of stimulation.

The following represents the course of the nitrite administration. The blood pressure being steady, the clockwork of the quick drum was started so as to bring it up to full speed before the cylinder was made to rotate by screwing up the friction wheel. The nitrite was then introduced into the side tube of the inhaler; an arrangement of valves permitted inspiration only to take place through this tube. The cylinder was started, and after a sufficient record of the pulse and respiration for the time being had been recorded, the nitrite was administered, the time of administration being recorded. A sufficient time having elapsed for inhalation, the air-tube of the inhaler was opened, the quick drum being permitted to run as long as was necessary for the purpose of recording the changes in pulse and pressure. During the recovery of pressure an occasional record of pulse and respiration was taken on the quick drum, corresponding marks being made on the slowly revolving cylinder.

It is well established that small doses of amyl nitrite cause a fall of blood pressure, resulting chiefly, if not entirely, from a powerful dilatation of the arterioles, reducing peripheral resistance to a great extent. Two distinct views have been advanced as to the cause of the dilatation. Filehne maintains that his experimental results demonstrate the dilatation to be due, not to a local action on the walls of the vessels, but to the direct action of the nitrite on the vaso-motor centres. On the other hand, Brunton, and also Mayer and Friedrich, believe they have shown that the dilatation is the result of a direct action on the walls of the vessel, and is independent of any effect on the central nervous system.

After discussing the experiments of Filehne, Brunton, and Mayer, an account is given of the experiments made by the authors to elucidate this question. These were made with cats, but control experiments with rabbits afforded the same results. In the first series the head of the animal was entirely cut off from the circulation, yet inhalation of pure amyl nitrite ($\frac{1}{8}$ th c.c.) caused a rapid fall of pressure, the lowest point reached exactly corresponding with that noticed in an immediately preceding experiment, in which the head was included in the circulation. In the second series all the arteries passing to the head were temporarily ligatured, and salt solution containing dissolved amyl nitrite ($\frac{1}{8}$ th c.c.) injected through the distal end of the carotid artery, one of the jugular veins being opened so as to admit of an escape of blood and hinder the production of a possibly abnormal intravascular tension in the brain. The same effect was constantly observed; the blood pressure rose, and not until the clamps were removed did the fall of pressure of the usual character occur. There is thus no indication of the characteristic nitrite effect, so long as the vessels are ligatured, although the nitrite must have passed to the medulla oblongata by vascular anastomosis, and therefore to the chief vaso-motor centre. By the injection of Berlin blue, it was demonstrated that access could be gained to the medulla through this channel. The conclusion that the nitrite effect is the result of an action on the vessels, and not on the central nervous system, was confirmed by observations on the effect produced by nitrites after splanchnic stimulation and section. Splanchnotomy is attended with a considerable reduction of pressure, and if nitrite be administered when this is at its minimum, a further reduction occurs, which, however, is not so great as that observed before section. But if administration of nitrite be delayed until the occurrence of one of the temporary elevations of pressure which are observed from time to time, the fall of pressure closely approximates to that produced before splanchnotomy. Simultaneous splanchnic stimulation and nitrite inhalation also cause a normal fall in pressure.

In experiments with the human subject, an accurate record was

taken of the pulse-rate, after inhalation of a known quantity of nitrite. A mask inhaler was specially devised, so as to avoid loss of substance during inhalation. It consisted of a conical metal box covering the mouth, and fitting accurately on the bridge of the nose by the aid of a hollow rubber border, which could be distended by injection of air. It is provided with three tubes opening out of a common trunk in the front of the mask; one of these was not furnished with any valve, but the two lateral tubes had each one valve, opening inwards and outwards respectively. The tube intended for the inspiration of nitrite had a continuation of india-rubber, in the middle of which a glass bulb was inserted for the reception of the nitrite. Spring clamps were placed on either side of the bulb. The mask having been adjusted to the face, and respiration being regular through the valvular tube, the drum was started at full speed so as to record the normal pulse rate, and the inhalation tube was opened by removing the clamps on either side of the bulb at the same time as the interior tube was closed. The time of inhalation was recorded by a signal marker.

There is a considerable variation on the part of individuals to nitrite effect, the acceleration of the pulse in the case of those of neurotic tendency being much greater, and the time of its continuance much less than in that of a lymphatic subject. The order of activity (extent of acceleration) for various nitrites deduced from a large number of experiments is (1) α -amyl; (2) β -amyl; (3) isobutyl; (4) secondary butyl; (5) primary butyl; (6) secondary propyl; (7) primary propyl; (8) ethyl; (9) methyl.

The action of each paraffinic nitrite has been closely contrasted with that of amyl nitrite. The results may be broadly summarised as follows:—

All the nitrites examined produce, in whatever way administered, a reduction of blood pressure, variable, however, according to the compound employed in its extent and in its progress, as well as in the ensuing recovery.

A pulse acceleration usually accompanies and succeeds the fall upon inhalation, the extent of inhalation varying in the case of individual nitrites. The acceleration is less upon intra-vascular injection, especially intra-arterial injection, than when administration is by inhalation; a distinct retardation of pulse is frequently produced by the former method, especially by carotid injection.

The extent of acceleration appears to be less in the case of cats than in the human subject.

The respiration is affected (1) temporarily during and immediately subsequent to inhalation, in various degrees by the different nitrites, and (2) permanently by the repeated administrations of the same or different nitrites.

As regards the principal effect, reduction of blood pressure, the activity (extent of reduction) of the various nitrites takes the following order when equal volumes are administered to animals by inhalation:—(1) secondary propyl; (2) tertiary butyl; (3) secondary butyl, (4) isobutyl, nearly equal; (5) tertiary amyl; (6) α -amyl, (7) β -amyl, nearly equal; (8) methyl; (9) butyl; (10) ethyl; (11) propyl.

The order is somewhat modified when the nitrites are given by intra-vascular injection. When the duration of the sub-normal pressure is considered, the order is nearly the reverse of that given above, the effect of methyl nitrite being the last, and that of secondary propyl nitrite one of the first, to disappear. In contrasting the results of the measurement of pulse acceleration produced by these nitrites, it is noticed that their activity in this respect does not follow the same order as that in reducing blood pressure, the amyl nitrites in particular occupying a higher position in the table. The causes of these differences will be considered in the second part of this paper, in conjunction with a discussion of the relation of the chemical constitution of the nitrites to the physiological effects now described, and also to those produced in striated muscle, a description of which will form part of the subsequent communication.

In order that the physiological data might be placed on an absolutely satisfactory basis for chemical discussion, we determined at the commencement of last year to repeat all the more important physiological experiments. This necessitated the labour of preparing fresh specimens of the nitrites. The results of these confirmatory experiments have been in every respect satisfactory, since they differed in no important respect from those previously obtained.

The chemical part of this enquiry has been conducted in the Research Laboratory of the Pharmaceutical Society, in London, whilst the physiological experiments have been made in the Pharmacological Laboratory of the University of Aberdeen.

V. "Some Points in the Structure and Development of Dentine." By J. HOWARD MUMMERY. Communicated by C. S. TOMES, F.R.S. Received February 7, 1891.

(Abstract.)

The purpose of the present paper is to show that there are appearances in dentine which suggest that it is formed by a connective tissue calcification, and that the process is more closely analogous to the formation of bone than has usually been supposed.

The varied theories held as to the structure and development of

dentine are partly due to the difficulties met with in the investigation of this tissue, soft and hard parts having to be retained in their natural relations to each other. Decalcification of the dentine by acids has been resorted to, a mode of preparing microscopical objects for study which is open to many objections. Sections cut by a process recommended by Dr. L. A. Weil, of Munich, exhibit the natural relations of pulp and tooth without the necessity of resorting to decalcification. Fresh specimens are fixed in sublimate, passed through gradually increasing strengths of spirit to absolute alcohol, and slowly impregnated with a solution of desiccated balsam in chloroform, dried with more balsam over a water-bath, and cut down on a stone with water. The present investigation was undertaken with the aid of this process, controlled by the examination of other specimens cut by the more ordinary methods.

Processes or bundles of fibres are seen, incorporated on the one side with the dentine, and on the other with the connective tissue stroma of the pulp; some of the bundles give evidence of partial calcification, reminding one of similar appearances in the calcification of membrane bone. Cells are seen included in the bundles and lying parallel to their course; these cells, it is concluded, form together with the odontoblasts the formative cells of the dentine, the calcification of which tissue should be looked upon as in part, at least, a secretion rather than a conversion process, the cells secreting a material which calcifies along the lines of and among the connective tissue fibres, the cells themselves not being converted into dentine matrix. These appearances are seen in the rapidly forming dentine of a growing tooth, as well as in more fully developed specimens. An examination of other Mammalian teeth reveals similar appearances. The dentine of the incisor of the Rat (*Mus decumanus*) shows with great distinctness the incorporation of the connective tissue fibres with the dentine, and there is a marked striation of the dentine near the pulp cavity, parallel with these fibres. The ivory of the Elephant's tusk shows the same relation of connective tissue to formed dentine. Vaso-dentine exhibits a very well defined connective tissue layer surrounding the pulp. This layer has hitherto been looked upon as consisting of odontoblasts, but this tissue shows no nuclei, and has the characters of a layer of flattened connective tissue fibres—a layer of nucleated cells in close apposition to the dentine, probably being the real odontoblasts of vaso-dentine.

Presents, March 5, 1891.

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March 12, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Right Hon. Lord Hannen, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

- “On the Plasticity of an Ice Crystal.” By the late J. C. McCONNEL, M.A. Communicated by R. T. GLAZE BROOK, F.R.S. Received January 24, 1891.

Two years ago, in the ‘Proceedings of the Royal Society,’ was published an account of some experiments on the plasticity of ice made by Mr. Kidd and myself. We proved the oft-repeated statement, that glacier ice is not plastic under tension, to be erroneous, and showed that any ordinary bar of ice composed of several crystals will yield continuously either to pressure or tension. But we found that a bar cut out of a single crystal with its length at right angles to the optic axis showed no signs of continuous stretching even under half the breaking tension, and other experiments convinced us that an ice crystal will not change its shape under either tension or pressure applied at right angles to its optic axis. These results seemed to render it highly probable that an ice crystal was not in any way plastic, and though after the winter was over we wished we had varied our experiments more, yet we quite expected that further investiga-

tion would only have corroborated the perfect "brittleness" of a single crystal.

Since our paper was written, my attention has been called to a passage in Professor James Thomson's masterly article on "The Lowering of the Melting Point of Ice by Distorting Stress" ('Phil. Trans.,' 1849), in which he expresses the opinion that crystals, whether of ice or other substances, are not plastic.

If we reject the idea of internal distortion of the crystals, we are driven to the conclusion that the observed plasticity must be due to some action at the interfaces, whereby the crystals alter their shape sufficiently to allow them to alter their relative positions. As to the nature of the action, various suggestions occurred to me. James Thomson explained the plasticity of ice at 0° C. by supposing the ice to melt at those interfaces where the stress was great, and the liberated water, after flowing to points where the stress was small, to again solidify. This might be extended to low temperatures by supposing a certain quantity of water to be kept in the liquid state by the pressure of residual impurities. But the process would be enormously retarded by the constant necessity for the distribution of salt being equalised by diffusion. Again, it is not clear how a bar of ice during this process would be able to resist a tension considerably greater than the pressure of the atmosphere. With more probability we may suppose one crystal to grow at the expense of another owing to the stresses and strains on the contiguous parts being different. Though the stresses were the same, the strains might be different, owing to ælotropic elasticity. But the elasticities are not likely to be very different in different directions, so for a very small extension of the bar we should expect considerable movement of the interfaces. There is, however, nothing to prevent the stresses being different. The tension in any direction parallel to the interface might be greater in one crystal than in the other. The migration of matter from one crystal to another under less stress would probably in almost all cases be accompanied by yielding to the external force producing the stresses. But in this case the effect would be very indirect, and again we might look for large movement of the interfaces.

Some such speculations had occupied my mind last autumn, and it was with considerable curiosity that I began experiments in December on the puzzling question of the real cause of the plasticity of ice. I took a bar of ice consisting of half a dozen crystals, made a drawing under the polariscope of the relative position of the interfaces, and then set up the bar with the ends supported and a weight hung from the middle. After two days, it had bent a good deal, yet, under the polariscope, I could detect no material change in the position of the interfaces. One crystal, however, had completely changed its appearance. It now strongly reminded me of a piece of unannealed

glass. There were two centres of colour encircled by irregular rings, and these remained much the same when the two faces through which the light passed were rubbed quite flat and the other crystals cut away. There could be no doubt that this crystal had suffered something more than mere elastic distortion.

The next experiment was very instructive. A thin slip of ice, being a single crystal, was subjected to bending stress as before, and left for several hours. It apparently bent very quickly, for after a few hours it was found crescent shaped, and luckily unbroken, lying at the bottom of the box. The optic axis was bent, and, though its change of direction was rapid where the bend was sharp, there appeared to be no break in continuity. On the other hand, the long narrow bubbles, which were originally no doubt parallel to each other and perpendicular to the slip, were still parallel to each other throughout. In fact, as I noted at the time, the crystal behaved as if it consisted of an infinite number of indefinitely thin sheets of paper, normal to the optic axis, attached to each other by some viscous substance which allowed one to slide over the next with great difficulty. This comparison proved to be the key to the whole question of the plasticity of a crystal of ice.

Further experiment showed that if a bar of ice consisting of a single crystal with the axis perpendicular to two of the side faces was subjected to bending stress, it would bend freely in the plane of the axis either at or below the freezing point, but not at all in a plane perpendicular to it. In the bent crystal the optic axis in any part was normal to the bent faces in that part. But any series of lines drawn in the substance of the ice which were originally parallel to the optic axis and to each other remained parallel to each other, though not, of course, to the optic axis. This was evidenced by the position of long narrow bubbles which frequently form at right angles to the planes of freezing, and also by the end faces of the bar remaining parallel to each other. When the optic axis was longitudinal, the bar bent indeed, but not very readily, and the general behaviour was more obscure. Still, this case, too, was in satisfactory agreement with the analogy mentioned above.

Let us state this analogy more fully. The sheets of paper offer no resistance to bending, but utterly refuse to stretch except, of course, elastically. Initially they are plane and perpendicular to the optic axis, and, after they have been deformed by bending, the optic axis at any point is still normal to the sheet at that point. They are of uniform thickness, whence it easily follows that the directions of the optic axis in any crystal form a series of straight, though not parallel, lines.

Detailed Account of the Experiments.

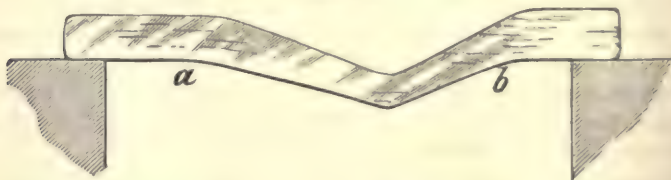
The first two experiments have been sufficiently described already.

The place of experiment was a north room in the Buol Hotel, Davos. A box without a lid was placed on a wooden table, and across the top of this box were laid two pieces of wood, which served to support the ends of the bar of ice. From the middle of the bar was suspended a weight with a loop of thick string. In the bottom of the box, but at the other end, *i.e.*, about a foot from the ice and 6 inches below it, was placed a registering thermometer of the Six pattern. Over the whole was put a thick wooden cover. As there was nothing inside the cover of great capacity for heat, I believe that any variation of the temperature of the ice was nearly simultaneously felt by the thermometer. This thermometer, which was used throughout, was divided into Fahrenheit degrees; its correction at freezing-point was tested both before and after the experiments. The error did not exceed $\frac{1}{4}^{\circ}$ F. At 6° F. I compared it with a spirit thermometer which had been verified at Kew; it read $\frac{1}{2}^{\circ}$ F. too high. These errors are negligible in the present work.

Exp. 3.—A bucket of water left in the ice room over night was found in the morning covered with ice about 15 mm. thick, consisting of several crystals. From this I sawed out a bar and planed it smooth and straight. The breadth was 10 mm.; the depth, 9 mm.

The bar contained many long bubbles in a vertical position. All the middle of it was one crystal with the axis nearly vertical. The two ends of the bar were composed of many crystals. A weight of 1.29 kilograms was applied from 11.20 A.M. to 8.30 P.M. on December 14. During this time the maximum temperature was $-2^{\circ}8$ C.; the minimum, $-5^{\circ}6$ C.; and the mean, about $-3^{\circ}6$ C. The bar had taken the shape of the diagram, fig. 1, which is copied from a trace

FIG. 1.

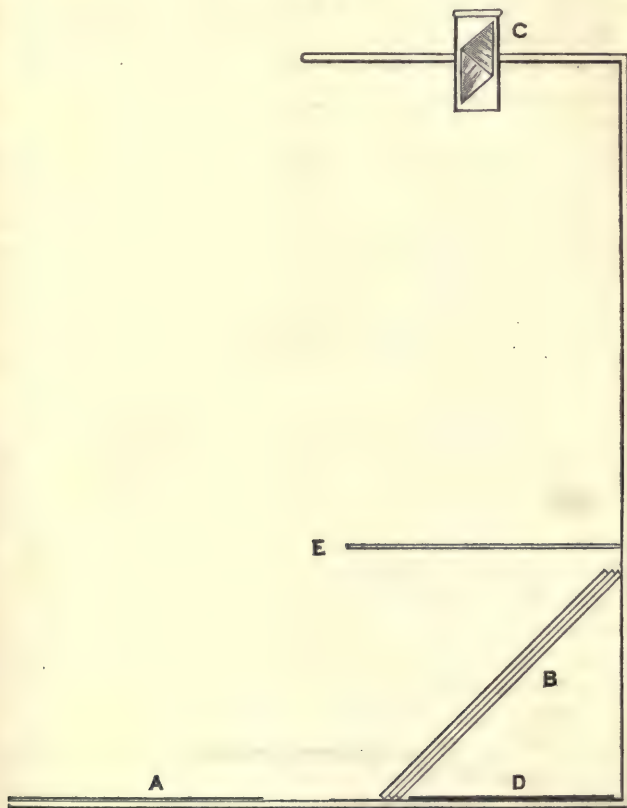


made soon after the experiment. The bends at the points indicated by *a* and *b* were more decided in the bar than in the trace. The exact position of the supports was not noted at the time, but they certainly did not extend right up to the bends at *a* and *b*. The fact that the two end pieces are still nearly in line suggests that the end surfaces

of the middle crystal are in the same position as before the bending. The question immediately suggested itself whether the bend was due to a limited number of layers sliding over each other by finite amounts, or to a true shearing strain. I examined the surfaces of the bubbles very carefully with a magnifying glass, and could find no trace of projecting edges or "faults," so I concluded it was a true shear. My polariscope was the same as was used two years ago.

Light from the white paper A, fig. 2, was reflected by the three

FIG. 2.



plates of glass, B, upwards through the Nicol C, and then the ice was laid on the glass stage E, or held in the closed hand. D was lined with black velvet. This simple apparatus served its purpose excellently, and it was seldom that I wished for a more elaborate apparatus with convergent light. The bent bar under this polariscope was found to have the optic axis as nearly as I could tell normal to the

bent faces throughout. If the black centre was near the middle of one half, the sharp bend was crowded with narrow coloured bands which moved slowly along as the bar was tilted, till as each band reached the straight piece beyond the bend it moved rapidly and broadened out.

The movement of the bands across the bend, though slow, was quite regular, so the direction of the optic axis changed quickly but not *per saltum*.

Exp. 4.—A similar arrangement. The bar was all one crystal except the parts actually on the supports. The optic axis was transverse, but horizontal. Depth, 9.5 mm.; breadth, 10 mm.; supports, 75 mm. apart. The weight of 1.29 kilos. was applied over 42 hours from 4.15 P.M. on December 15 to 10.35 A.M. on December 17. The minimum temperature was $-7^{\circ}8$ C., the maximum $-1^{\circ}1$ C., the mean about $-3^{\circ}3$ C.

Decided evaporation had taken place; the edges of the bar were rounded and the string which had stuck to the bar was raised on ridges. The greater part of the bar was $8\frac{1}{4}$ mm. deep, 9 mm. broad. In comparing the traces taken before and after the experiment I could find no bending. It certainly did not amount to half a millimetre. The traces were taken by laying the bar on a sheet of paper and following the upper and lower edges with a pencil.

Exp. 5.—The same bar, turned so as to put the optic axis vertical, bent rapidly.

Depth $8\frac{1}{4}$ mm., breadth $8\frac{1}{4}$ mm. Distance between supports 73 mm. The weight of 0.62 kilo. was applied from 11.10 A.M. to 9.5 P.M. on December 17. The minimum temperature was $-4^{\circ}4$ C., the maximum $-1^{\circ}7$ C., mean about $-3^{\circ}0$. The depression of the middle measured on the trace was about 4.4 mm., which had taken place in 10 hours. Assuming that in Experiment 4 the depression was less than 0.5 mm., the bending of the bar in the new position must have been at least thirty-seven times as fast. It is true the depth and breadth were slightly less, but the weight was less than half as great. The results of *Exp. 3* as to bubbles and optic axis were confirmed.

Exp. 6.—A bar with the axis longitudinal.

I obtained a large lump of thick ice from the Davos lake, and from this cut a bar which appeared to be all one crystal, with the axis longitudinal. I need not enter on the details of the experiment, especially as the temperature rose above freezing-point. But at the end the bar had the shape shown in the diagram, Figs. 3, 4.

The dotted line indicates a division between the crystals. The double-headed arrows show the direction of the optic axis in different parts, or at least the projection of that direction on the plane of the paper. This was determined by making the field of the polariscope as dark as possible, putting the part of the bar in question in the middle

FIG. 3.

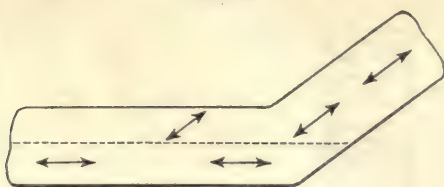
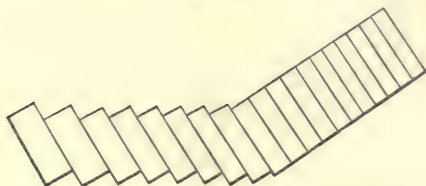


FIG. 4.



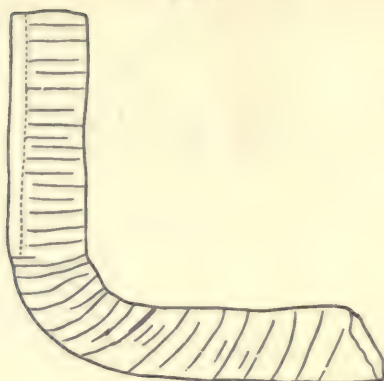
of the field and then turning it till it looked as dark as possible. When this is done the axis lies in the principal plane of the instrument. It will be noticed that in each crystal the direction of the optic axis is almost uniform. I imagine that the two crystals existed virtually in the bar, but that their optic axes were so nearly parallel that in the polariscope they behaved as one crystal. The kind of shear that must have taken place in the upper crystal is represented in fig. 4 by a number of layers of finite thickness slipped over one another.

I cannot say definitely that the bending was either slower or faster than in a bar all one crystal with the axis vertical.

The part beyond the dotted line is perhaps due to the intrusion of another crystal completely overlapped by the main crystal, or perhaps to some alteration of the optical qualities due to elastic strain.

Exp. 7.—Another bar cut from the same lump was a single crystal with the axis nearly longitudinal, inclined perhaps at 5° to the side of the bar. Breadth 10·7, depth 10·5, distance between supports 84 mm., weight 1·29 kilos. After six hours, during which time the temperature had been between $-1^\circ\cdot7$ and $-0^\circ\cdot6$ C., the bar was found lying at the bottom of the box broken into two pieces. It had bent so much that it must have slipped down between the supports and been broken in the fall. The two parts could be accurately pieced together. At the dotted line there was a very rapid but not sudden change in the direction of the optic axes. The shape of the surfaces normal to the optic axes is shown in fig. 5 (p. 330). These sliding surfaces must have the geometrical property that the normal drawn at any point to any point is also normal to all the surfaces it cuts within the bar. It is in fact parallel to the optic axis all along its course.

FIG. 5.



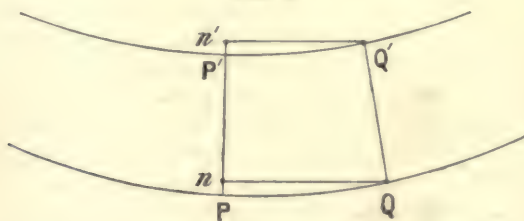
It will be noticed that the directions of the optic axis in different parts form a series of straight lines. This is an immediate consequence of the hypothesis of the existence of sliding surfaces, and may be shown in the following way.—In the part of the crystal beyond the dotted line, however, this rule does not hold good.

In the original unstrained crystal the optic axis is in the same direction everywhere. Hence layers perpendicular to it are of equal thickness throughout. Their subsequent bending and slipping does not affect their uniformity of thickness.

We need only consider one of the principal directions of curvature.

Draw PP' , QQ' normal to the surface at P , Q , to meet the next surface at $P'Q'$. On PP' drop perpendiculars Qn , $Q'n'$. All the quantities in small distances are small except the radii of curvature ρ , ρ' at P and P' . Since the thickness of the layer is uniform, $PP' = QQ' = nn'$. Thus to the second order of small quantities $Pn = P'n'$.

FIG. 6.



But since Pn is normal to the curve PQ at P , $Pn = Qn^2/2\rho = Q'n'^2/2\rho'$ to the second order. Now this would have been the

expression for $P'n'$ if it had been normal to the curve $P'Q'$ at P' and $Q'n'$ had been drawn perpendicular to it, and PP' is normal to the curve $P'Q'$ at P' .

Exp. 8.—This was an experiment on a bar composed of three crystals designed to investigate the action at the interfaces of crystals. The bar bent a good deal, but nearly the whole bend occurred in the middle of one of the crystals. I had cut nicks in the sides of the bar to test for migration of the interfaces within the ice, but found none. It appears, in fact, that the interfaces do not in any way assist the plasticity, but hinder it by fettering the sliding of the layers in the separate crystals.

Exp. 9.—Out of some thick ice formed on the surface of the water in a foot-bath I cut a bar which was all one crystal. When the bar was in position the optic axis was horizontal, and inclined at about 60° to the length of the bar.

Breadth, 13 mm.; depth, 11.7 mm.; distance between supports, 38 mm.

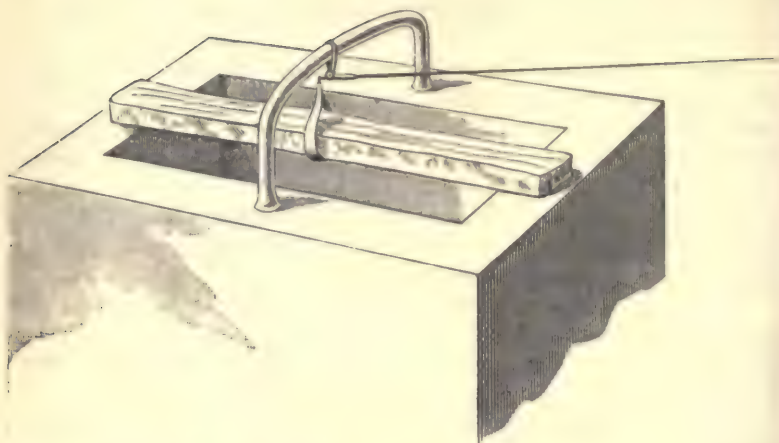
A weight of 1.29 kilos. was applied for $16\frac{1}{2}$ hours from 4.50 P.M. on January 29 to 9.15 A.M. on January 30, during which time the maximum temperature was -5° C., the minimum -12.7° C., and the mean about -8.6° C. The depression of the middle was 1.9 mm. A little consideration will show that by the theory of the sliding layers, the upper and lower surfaces of the bar should be bent in such a manner as to still contain straight lines perpendicular to the optic axis. Some such deformation was observed, but it was not very definite. I noticed that the numerous bubbles which were originally parallel to the axis were still parallel to the upper and lower surfaces in their neighbourhood.

I now set up an arrangement for obtaining more accurate measurements of the rate of bending. A large square aperture in an iron plate was bridged by a curved iron bar rigidly attached to the plate. The bar of ice was placed across the aperture. A loop attached to the curved bar supported a wire lever, of which the long arm served as a pointer on a scale, and the short arm carried a stirrup which embraced the ice. When the bar bent the stirrup was depressed and the pointer raised about twenty-eight times as much.

This part of the apparatus was placed in a cigar box, at one end of which the pointer projected through a slit, while there was a hole in the bottom to allow the string, to which the weight was attached, to pass through. The Six thermometer was on a level with the ice, and could be read by gently lifting the lid without disturbing anything. The mirror and scale with which the position of the pointer was read were fastened to the box.

The only part of the stirrup that touched the ice was the flat piece of tin at the bottom. This was slightly roughed and made flat, so

FIG. 7.



that it should not slip off the projection left by the gradual evaporation of the unsheltered surface.

The string carrying the weight was put as close as possible to the stirrup without risk of touching it, and so that the central point of the aperture came somewhere between the two.

Exp. 10.—The bar was taken from the same bath ice as in the last experiment. It was all one crystal with the axis vertical. The first attempt was a failure, owing, I believe, to some snow getting underneath the iron plate, and, by giving way gradually, tilting up the plate. I had put a good deal of snow inside the cigar box, with the hope of preventing evaporation. This made the readings erratic and unreliable, so the next day I turned the bar over to give the stirrup a smooth surface to bear upon, and started fresh. The results are given in the table (p. 333). I think the amount of depression may generally be trusted to within 0.01 mm.

Several interesting points are brought out in this table. When the weight is changed, the alteration in the rate of depression is great out of all proportion, *e.g.*, the alteration from 0.0058 to 0.410 when the weight is changed from 0.174 to 1.47 per square cm. During the course of the experiment there was a decided rise in plasticity; compare the earlier with the later rates under 1.47 per square cm. at similar temperature. This is corroborated by the increase of speed under 0.85 kilo.

The only exception, *viz.*, the decrease of speed at first under 41.7 kilos. was due, I believe, to the elastic strains which had been set up in the preliminary bending. The effect of these elastic strains is shown by the undoubted rise of the middle of the bar when the weight was removed at the end of the experiment.

Table I.—Ratio of magnification, 28. Average breadth, 14.2. Depth, 12. Area, 1.7 sq. cm.

Exp. 10.

Time.	Apparent extension.	Real extension.	Interval in hours.	Rate in mm. per hour.	Weight in kilos.	Kilos. per sq. cm.	Temperature.	
							Maximum.	Mean.
Jan. 30.								
9.30 A.M.	3.3	0.118	0.5	0.236	2.5	1.47	-6.7	-6.7
10.0 "	2.5	0.089	0.517	0.172	"	"	-7.2	-7.2
10.31 "								
10.33 "	2.4	0.086	1.17	0.0735	1.445	0.85	-6.1	-6.7
11.43 "								
11.45 "	3.6	0.128	1.17	0.109	"	"	-6.1	-6.7
12.55 "								
12.59 "	-3.5	-0.125*	1.07	-0.117	0.915	0.54	-7.2	-7.8
2.3 P.M.	+3.5	+0.125	1.87	0.067	"	"	-8.3	-8.9
3.55 "								
4.3 "	+2.4	+0.086	1.48	0.058	"	"	-9.2	-10.0
5.32 "								
5.35 "								
Jan. 31.	+2.4	0.086	14.8	0.0058	0.295	0.174	-10.0	-15.0
8.25 A.M.								
8.28 "	9.2	0.328	0.8	0.410	2.5	1.47	-7.8	-8.9
9.16 "								
9.18 "	-1.3	-0.046	0.3	-0.153	0.0	0.0	-7.8	-7.8
9.36 "	-0.6	-0.021	0.97	-0.022	"	"	-6.7	-7.2
10.34 "								

* This was almost certainly a mistake in the reading.

Table II.—Ratio of magnification, 28. Breadth, 13.7. Depth, 11.95. Area, 1.63.

Exp. 11.

Time.	Apparent extension.	Real extension.	Interval in hours.	Rate in mm. per hour.	Weight in kilos.	Kilos, per sq. cm.	Temperature.	
							Maximum.	Mean.
Jan. 31. 12.30 P.M.	-0.5	-0.018	8.33	-0.00215	2.5	1.53	-7.5	-8.9
8.50 "	-0.3	-0.011	13.3	-0.00083	"	"	-11.1	-17.2
Feb. 1. 10.8 A.M.								

Into this matter I enter more fully below.

The indicated rise between 12.59 and 2.3 P.M. is, I feel sure, simply due to a misreading. Whenever the weight was altered the apparatus was unavoidably disturbed, so I had to take an entirely fresh reading of the pointer. Generally this only differed by fractions of a millimetre from the previous reading, but in the case in point it was nearly 6 mm. greater. The ice showed an inconvenient tendency to slip backwards on the iron plate, thus bringing the end of the pointer forwards till it almost touched the edge of the slit. The ice had to be pushed forwards three or four times during the experiment. Of course a fresh reading was taken after each such displacement, so that no error resulted. This trouble was caused doubtless by the plate not being accurately level. In subsequent experiments I was more successful in avoiding it.

Exp. 11.—I desired to establish with the more delicate system of measurement that the plasticity is inappreciable when the bending stress is applied at right angles to the axis. I cut a bar, all one crystal, from the bath ice, and planed it so that the upper and lower surfaces were as accurately as possible parallel to the optic axis. In the polariscope, when the middle of the black cross was in the middle of the bar, the two faces were equally inclined to the lines of sight. I then set up the apparatus in the usual way. The results are seen in the annexed table (p. 334).

It will be seen that the pointer indicated a rise of the stirrup amounting in the $21\frac{1}{2}$ hours to 0.29 mm. As was before mentioned, the stirrup was slightly roughed to prevent it from slipping, so at first it would make contact with the bar at only a few points. Evaporation would help to extend the contact to large surfaces, and admit of a slight movement of the stirrup relatively to the ice. Thus the experiment was not as satisfactory as could be wished. It is possible that a very slight depression of the bar might be masked by this effect of evaporation. But even supposing that the rate of real depression was twice as great as that of the apparent elevation, viz., 0.0043 mm. per hour, it would still be very small compared with the rates of the next experiment. I am at any rate entitled to say that within the limits of error of experiment there is only one kind of plasticity in an ice crystal, viz., that due to the sliding layers at right angles to the optic axis. It is probable that the same source of error was active in other experiments, but in them the effect would be almost negligible.

Exp. 12.—The same bar was turned on its side so that the optic axis was vertical.

Table III.—Ratio of magnification, 2. Breadth, 11.28. Depth, 13.2. Area, 1.48.

Exp. 12.

Time.	Apparent extension.	Real extension.	Interval in hours.	Rate in mm. per hour.	Kilos. per sq. cm.	Temperature.	
						Maximum.	Mean.
Feb. 1. 10. 15 A.M.	0.9	0.032	0.7	0.0457	1.69	-14.4	-15.3
10. 57 "	7.6	0.272	2.33	0.117	"	- 8.9	-11.1
1. 17 P.M.							
1. 21 "	7.3	0.26	0.683	0.38	"	- 6.1	- 7.0
2. 2 "	4.4	0.157	0.267	0.59	"	- 4.7	- 5.6
2. 18 "							
2. 20 "	7.2	0.257	0.275	0.97	"	- 3.3	- 3.9
2. 36½ "	7.2	0.257	0.192	1.34	"	- 2.2	- 2.8
2. 48 "	4.6	0.164	0.108	1.52	"	- 2.0	- 2.2
2. 54½ "	5.7	0.204	0.108	1.89	"	- 1.1	- 1.7
3. 1 "							
3. 4 "	-0.4	-0.014	0.183	-0.076	0.0	- 2.5	- 3.3
3. 15 "	-0.4	-0.014	0.517	-0.027	"	- 3.6	- 4.7
3. 46 "							
3. 47 "	8.7	0.31	0.262	1.18	1.69	- 5.8	- 6.1
4. 1½ "							
4. 4 "	-2.9	-0.104	4.12	-0.0252	0.0	- 6.4	- 9.7
8. 11 "							
8. 14 "	8.5	0.304	0.408	0.745	1.69	-13.0	-13.0
8. 38½ "	12.3	-0.44	0.642	0.685	1.69	-13.0	-13.2
8. 17 "							
9. 21 "	-2.0	-0.072	11.8	-0.0061	0.0	- 6.7	- 7.8
Feb. 2. 9. 10 A.M.							

We first notice that the plasticity exists down to $-14^{\circ}4$. At this temperature the bending was slow, but this was due in great part to the fact that it came at the beginning, and the bar was as usual. The rapid growth of plasticity, independently of the temperature, is shown by the rate of 0.59 mm. per hour at a mean temperature of $-5^{\circ}6$, being raised in less than two hours to 1.18 mm. at $-6^{\circ}1$. The tendency to recover when the weight is removed is shown three times over in the table. As might be expected, it soon becomes very slow, and in that case after twelve hours, when the recovery amounts to 0.72 mm., it has probably stopped altogether. In the fall of rate from 1.89 at $-1^{\circ}7$ to 1.18 at $-6^{\circ}1$ and 0.745 at -13° , in spite of the natural tendency for the rate to rise, we seem to have a real effect of temperature. After 8.38, the cigar box had to be left open as the pointer had almost reached the lid of the box, and so the subsequent temperatures are unreliable. I imagine that the change from 0.685 to 0.745 was due to a fall of temperature.

At the beginning of Exp. 11 the bar measured 14 mm. by 12.3 mm., which was reduced at the end of Exp. 12 to 13 mm. by 10.8 mm. The evaporation had been rather more rapid just at the bend of the bar. This was owing, I believe, to the circulation of air through the hole by which the string passed out.

I measured the total depression on the trace as 2.6 mm. As measured by the pointer it is 2.45. The agreement is as good as could be expected.

Exp. 13.—In this experiment I used a thicker bar and tried a variety of weights. The bar was only just small enough to go into the stirrup. (See Table IV, next page.)

The stiffness of the bar in the first three hours is surprising.

Exp. 14.—In all the experiments hitherto on bars composed of single crystals it happened that the optic axis had been vertical when the ice was formed, so that the planes of freezing coincided with the sliding layers. I fully believed that this coincidence was merely accidental, and what happened in Exp. 8 had confirmed this idea, but I thought it desirable to have a more direct proof. So I cut a piece out of a good large crystal in the ice, found on the surface of the water in the bucket, in which the optic axis was not vertical. When the bar was put in position the planes of freezing were vertical and parallel to the length, and the optic axis was normal to the length and inclined at about 50° to the vertical. The bar was about 8 mm. square, and the distance between the supports was 51 mm.

Under a weight of 0.62 kilo. in 4 hours 28 minutes at a mean temperature of $-4^{\circ}4$ (the maximum $-1^{\circ}4$) it bent downwards about 4 mm. There was a large lateral bend, which made the vertical bend very difficult to measure.

If the sliding layers had been necessarily the same as the planes of

Table IV.—Ratio of magnification, 26. Depth, 17½. Breadth, 15.7. Area, 2.75.

Exp. 13.

Time.	Apparent depression.	Real depression.	Interval in hours.	Rate in mm. per hour.	Weight in kilos.	Kilos. per sq. cm.	Temperature.	
							Maximum.	Mean.
Feb. 2. 10.40½ A.M. 2.35½ P.M. 4.0 "	3.8	0.146	3.92	0.0872	2.5	0.91	-3.9	-5.0
	5.9	0.227	1.41	0.160	"	"	-3.9	-5.0
4.1 5.33½ "	2.2	0.084	1.56	0.054	1.29	0.47	-6.1	-6.7
5.35 6.2½ "	1.9	0.073	0.46	0.159	2.5	0.91	-7.2	-7.5
6.6 6.30 7.33½ "	3.1	0.119	0.40	0.297	3.79	1.38	-7.8	-8.0
	8.9	0.342	1.06	0.323	"	"	-8.0	-8.9
7.36 8.20½ "	3.8	0.146	0.74	0.197	2.5	0.91	-9.7	-10.0
8.26 Feb. 3. 9.16 A.M.	7.5	0.288	12.83	0.0225	0.62	0.225	-6.4	-7.8
9.18½ 9.54 "	4.3	0.165	0.59	0.28	2.5	0.91	-6.1	-6.1
9.57 10.21½ "	6.1	0.235	0.41	0.57	3.79	1.38	-5.9	-5.9

freezing, this bar should not have bent at all. If, however, the sliding layers are necessarily perpendicular to the optic axis, this bar should have been free to bend on the plane of the optic axis, but not in the perpendicular plane. In the experiment the plane of the total bend contained the optic axis. Thus the experiment was decisive.

In attempting to discover the manner in which the rate of the molecules sliding over each other depends on the driving force, we are met by the difficulty that the rate of depression depends on at least three other circumstances, the temperature, the previous history of the bar, and the irregularity of the stresses and strains within the bar. The second is to some extent avoided by only considering the rates observed immediately before and immediately after the change of weight. The third is probably not very important. In the following table are collected all the instances which occurred, with the attendant changes of temperature. The changes of rate are not so great as the square, but greater than the first power of the changes of the applied force. In the table may be seen the amount of correspondence with the power $\frac{3}{2}$. The two most glaring discrepancies are in the second

Table V.

Change mean temperature.	Change of weight in kilos. per sq. cm.	$\left(\frac{\text{Old force}}{\text{New force}}\right)^{\frac{3}{2}}$	Change of rate in mm. per hour.	Ratio of rates.
— 7.2 to — 6.7	1.47 to 0.85	0.442	0.172 to 0.0735	0.427
— 10.0 to — 15.0	0.54 to 0.174	0.182	0.058 to 0.0058	0.100
— 15.0 to — 8.9	0.174 to 1.47	24.6	0.0058 to 0.410	70.7
— 5.0 to — 6.7	0.91 to 0.47	0.373	0.160 to 0.054	0.338
— 6.7 to — 7.5	0.47 to 0.91	2.70	0.054 to 0.159	2.95
— 7.5 to — 8.0	0.91 to 1.38	1.87	0.159 to 0.297	1.87
— 8.9 to — 10.0	1.38 to 0.91	0.537	0.323 to 0.197	0.610
— 10.0 to — 7.8	0.91 to 0.225	0.122	0.197 to 0.0225	0.114
— 7.8 to — 6.1	0.225 to 0.91	8.15	0.0225 to 0.28	12.4
— 6.1 to — 5.9	0.91 to 1.38	1.72	0.28 to 0.57	2.04

and third instances given in the table, when the power 2 is well satisfied. But these discrepancies may be largely, if not entirely, explained by the great change of temperature. Without elevating the statement to the rank of a law, we may say that fairly close agreement with the observed facts is obtained by supposing that when the molecules of ice slide on each other the cube of the friction varies as the square of the velocity.

In attempting to pass from the rate at which the centre of a loaded bar sinks to the coefficient of plasticity, we meet with considerable difficulties, and shall have to content ourselves with a rough approximation. It might well be thought that the problem of a rectangular

elastic bar, supported at either end and loaded in the middle, had been fully worked out. But this does not appear to be the case. The ordinary elementary treatment makes the gigantic assumption that plane cross-sections of the unbent bar remain plane, and that the lateral contraction or expansion of elementary strips parallel to the length of the bar under longitudinal pulls or thrusts are the same as in free space. It does not consider any shearing stresses or strains. It is true that Rankine (*'Applied Mechanics,'* p. 338), assuming the results of this method, proceeds to find an expression for the shearing stress. He makes it proportional to $a^2 - x^2$, where the origin is at the centre of the bar, the axis of x is drawn upwards, and $2a$ is the depth of the bar. But this expression is inconsistent with the general equations of an elastic solid. St. Venant's solution of the bending of a bar, given in Thomson and Tait's *'Natural Philosophy,'* postulates equal and opposite couples applied at the two ends, so that the bending moment is uniform throughout. The importance of the absence of this uniformity is not trifling but fundamental, for in our case everything depends on the shears, and in St. Venant's solution there are no shears.

I fancy that I see my way to obtaining the complete solution in the form of infinite series. But, since it ceases to be applicable the moment plastic strains take place, it would only enable us to determine the initial stresses, and this would hardly justify the insertion here of such a long investigation.

The following simple but imperfect treatment must suffice. Let us first define the coefficient of plasticity. Take a rectangular element with two faces normal to the optic axis, and let these faces be subjected to a tangential force U per unit of area in opposite directions, parallel to another pair of faces.

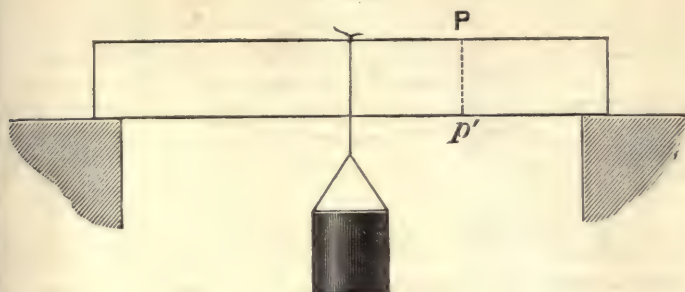
FIG. 8.



Then if the rate of growth of two of the angles, or rate of diminution of the other two be denoted by $d\chi/dt$, the coefficient of plasticity p may be defined by the equation

$$\frac{d\chi}{dt} = pU \dots\dots\dots (1).$$

FIG. 9.



The bar is represented in fig. 9, with a weight W hanging from the middle. The length between the supports is l , the breadth b , the depth d . U is the force per unit area which acts on a small vertical interface in a vertical direction, and when U is positive the matter to the left of the interface is urged upwards. The force per unit area on a horizontal interface in a direction parallel to the length of the bar is necessarily the same, and is also denoted by U . Consider the equilibrium of the part of the bar to the right of any cross section PP' . It is urged upwards at the support by a force equal to $\frac{1}{2}W$; therefore, if we neglect its weight, the total vertical force on the section PP' is also $\frac{1}{2}W$.

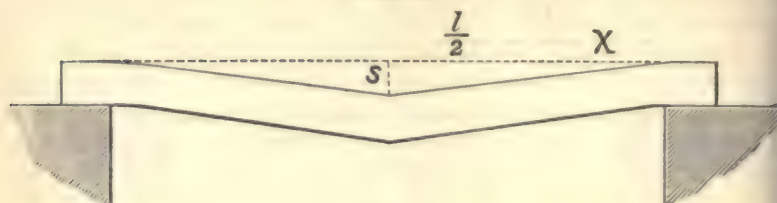
If $\bar{U}$ be the average of U over the section

$$bd\bar{U} = \frac{1}{2}W \dots\dots\dots (2).$$

U cannot be constant over the section, for it necessarily vanishes at the upper and lower surfaces of the bar.

The average shear over any cross section being the same, except that the sign suddenly changes at the middle of the bar, it is reasonable to suppose that the same amount of plastic shearing strain would take place between the layers perpendicular to the optic axis at every cross section. This condition makes the bar bend sharply when the weight is applied, and keeps the two halves straight. In the earlier experiments, where the bending was considerable, this form was observed before its cause had been perceived. For this form to be assumed without elastic strain, the plastic strain must be the same, not merely in corresponding points of different cross sections, but also throughout each cross section itself, and, in fact, throughout the entire half of the bar. But as we have seen, the shearing stress must vanish at both the upper and lower surfaces. Doubtless the truth is that the state of shearing strain is nearly uniform throughout the bar, except close to the surface, where it rapidly diminishes to zero. Probably in these regions the elastic strains are very great, and quite different from what they are elsewhere.

FIG. 10.



Let s be the depression of the middle of the bar, χ the angle either half makes with the horizontal. We have $s = \frac{l}{2}\chi$. When χ is small,

$$2 \frac{ds}{dt} = l \frac{d\chi}{dt} = lp\bar{U} = lp \frac{W}{2bd},$$

and

$$p = \frac{4bd}{lW} \frac{ds}{dt} \dots \dots \dots (3).$$

This gives the coefficient of plasticity in terms of the unsupported length of the bar, the weight per unit area of cross section, and the observed rate of depression. We have employed equation (2), which is strictly applicable only when the bar is straight and horizontal. But, in the cases to which we have to apply these results, χ is so small that the error is negligible. It was hardly worth while calculating the numerical value of p , especially as it has been shown to depend on the temperature, on the value of $\bar{U}$ nearly, and also on the previous history of the bar. But the above investigation will assist any one in estimating, as far as can be done from my experiments, the rate of distortion of an ice crystal in any given case.

In several cases in the experiments, after a heavy weight was removed, a slight gradual unbending of the bar took place. At first I thought this a mere consequence of the irregular elastic strains on the bar, the parts most severely strained gradually bending back the rest. But the magnitude of the recovery seems, on closer examination, to put this explanation out of the question, and I have now little doubt that it is a true molecular effect.

In Exp. 12, after a stress of 1.69 kilos. per sq. cm. had been removed, the middle of the bar rose 0.0104 cm. in four hours. According to an experiment by Moseley ('Phil. Trans.,' 1871), Young's modulus for ice is 92,700 kilos. per sq. cm. Hence, if we neglect the effect of the plastic strains in one bar of ice, the elastic depression under 2.5 kilos. should have been 0.00138 cm., less than one-seventh of the recovery observed. The permanent or plastic strains in Moseley's bar are considerable, so that the deduced value of Young's modulus may be too great. Bevan, also by flexure of bars

of ice, found the value 60,000. Reusch ('Nature,' vol. 21, p. 504), by experimenting on the sonorous vibrations of rectangular plates of ice, found Young's modulus to be 23,632 kilos. per sq. cm. (this last method seems rather dangerous). In attempting to devise an imaginary system of strains sufficiently great to render such a recovery as 0.1 cm. possible, we are soon brought up by the breaking tension of ice. Direct experiments by Moseley give this as 7 or 8 kilos. per sq. cm., and Kidd and myself found it in one case to be 8.3 kilos. per sq. cm., but the fact that the bar of ice in Exp. (11) bore the weight of 2.5 kilos. before any plastic strains had taken place brings it out greater than 15.5 kilos. per sq. cm., and the bar in Exp. (13) was able to endure an even greater stress.

A similar discrepancy has been noticed in the case of cast iron (Rankine, 'App. Mechanics,' § 297).

Using the latitude given by the uncertain values of the constants to the utmost, I have not been able to devise any system of elastic strains which could possibly make the bar rise 0.01 cm., and there is no reason to suppose that the unknown system of strains actually occurring in the experiments would be exceptionally well adapted to such a purpose. I conclude, then, that we have to deal with a real tendency of the forcibly displaced sliding layers to slide back. The rate of recovery, rapid at first, soon falls off. Thus in Exp. (10) there was a recovery of 0.046 mm. in the first 18 minutes, and only 0.021 in the next 58. In Exp. (15) after 0.014 in the first 11 minutes, and the same in the next 31, the motion probably came to a standstill after a few hours, practically, if not absolutely. Thus in Exp. (12) the bar was left with no weight on for 12 hours, and the recovery was only 0.072 mm.

[Mr. McConnel died suddenly at Davos while engaged on the foregoing paper, which has been printed from his rough copy with some few alterations of no great importance. I thought it better to do this than to attempt to edit it; though I know from his last letters to me that the author would have himself, if he had lived, been able to leave it in a more finished state than that in which it now appears. —R. T. G.]

II. "On the Effect of Temperature upon the Refractive Index of certain Liquids." By W. CASSIE, M.A. Communicated by Professor J. J. THOMSON, F.R.S. Received February 19, 1891.

In my paper "On the Effect of Temperature on the Specific Inductive Capacity of a Dielectric" ('Phil. Trans.,' A, 1890), the

values obtained for the temperature-variation of specific inductive capacity of four of the liquid dielectrics investigated were compared with the corresponding values of the temperature-variation of refractive index found by Messrs. Dale and Gladstone.* And the relations between these two quantities, though not in accordance with Clerk Maxwell's electromagnetic theory of light, were near enough to make it worth while to try whether the divergence from theory might not be due to differences in composition. Accordingly I measured the rate of change of refractive index with temperature for the same specimens of the liquids as were used in the electrical experiments. In the case of olive oil, however, the original supply could not be found. The results obtained are very close to Messrs. Dale and Gladstone's for those of the liquids they had examined, and for the others the optical effect shows a similar divergence from Maxwell's theoretical relation. And considering the enormous difference in the rapidity of the electrical and optical effects, this is not surprising.

The change of refractive index was measured by observing with a spectrometer the minimum deviation of the D lines for a bottle prism filled with the liquid. The observations were taken at two temperatures, viz., that of the room, 16° or 17° C., and a higher temperature, about 40° C., obtained by heating the prism and its contents in warm water. The results are shown in the following table, the last column giving the values of Messrs. Dale and Gladstone:—

	Rate of change per degree centigrade of		
	Specific inductive capacity.	Refractive index.	Refractive index (D and G).
Turpentine	— '0012	— '0003	— '00033
Carbon bisulphide.....	— '004	— '0006	—
Glycerine.....	— '006	— '0002	— '00018
Benzoline.....	— '0006	— '00037	— '00037
Benzine.....	— '0014	— '00043	— '00042
Paraffin	+ '0023	— '00017	—

In the case of glass, the change of refractive index with temperature was found by Stefan† to be 0·0000023 per degree centigrade, a quantity of quite a different order from 0·002, the rate of change of specific inductive capacity. And in view of the influence of the time of charging, even when extremely short, upon the specific inductive

* Results collected in Watts's 'Dict. of Chem.,' vol. 3.

† 'Wien., Akad. Sitzber.,' vol. 63, Abth. 2.

capacity of glass revealed by Professor J. J. Thomson's experiments,* this is only what might be expected.

III. "On the Bisulphite Compounds of Alizarin-blue and Coerulein as Sensitisers for Rays of Low Refrangibility."
By GEORGE HIGGS. Communicated by LORD RAYLEIGH,
Sec. R.S. Received February 19, 1891.

The determination of the relative wave-lengths of the Fraunhofer lines, by photographing all the orders of spectra given by any particular grating, includes certain subjects which present more or less difficulty, and that of selecting or producing a dye-bath adapted to the requirements of the two or more orders comprising the subject is intimately connected with that of the choice of absorbing media.

Having been engaged for some time in investigations of this nature, I had occasion, during the summer of 1889, to require an impression of the 2nd order, about λ 3300, contiguous with that of the red end of the 1st order, and finding that the ordinate of an actinic curve for a plate immersed in a very dilute alcoholic ammoniacal solution of cyanin (1 : 30,000), reduced to about one-fourth of that for an unprepared plate, I abandoned its use for this purpose. The results appeared to be unaffected by the addition of quinine.

Subsequently, induline, coerulein, alizarin-blue, and the bisulphite compounds of the two latter were used. When obtained in a state of sufficient purity the alizarin-blue S leaves little or nothing to be desired, for, whilst possessing, in a high degree, sensitising properties for rays throughout the region comprised between λ 6200 and 8000, it does not, like cyanin, lower the sensitiveness to the violet and ultra-violet.

The following is one of the processes I employed in the preparation of the dye-stuff in a pure state:—

To a saturated solution of sodium bisulphite in a mortar is added alizarin-blue paste. This is disintegrated with a pestle, and poured into a glass vessel capable of holding an additional quantity of sodium bisulphite, in all 10 parts of the paste to 20 parts of bisulphite, and another 10 parts of water. The vessel is well stoppered, set aside in a cool place for five or six weeks, and shaken daily, but left undisturbed during the last eight or ten days.

The solution is decanted, filtered, and treated with alcohol, to precipitate the greater portion of the remaining sodium bisulphite. 50 parts of water are now added with a sufficiency of sodium chloride to form a concentrated solution. Again set aside in an open-mouthed

* 'Roy. Soc. Proc.,' vol. 46.

glass jar, covered with bibulous paper, for seven or eight days, a deposition of the dye in a crystalline state, together with sulphite of calcium, will take place, which latter, owing to its insolubility in water, may be removed by filtration.

The alizarin-blue S is separated from any unaltered substance left in the original stoppered vessel by solution, and added to the brine, now purified from lime salts, and once more set aside to crystallise, the final purification being effected in a beaker containing alcohol and a small percentage of water to remove the last traces of sodium chloride, collecting the crystals on a filter-paper and drying at ordinary temperatures.

The needle-shaped crystals are of a deep-red. Dilute solutions are of a pale sherry colour, changing, with the addition of a few drops of ammonia, to a green, which immediately gives way to magenta and every shade of purple, till oxidation is complete, when it assumes a blue colour, the absorption spectrum of which is continuous and strongest in the least refrangible end, presenting the appearance of extending into the infra-red.

Plates immersed in a solution containing 1 : 10,000 and 1 per cent. of ammonia give the most perfect results the day after preparation, but rapidly deteriorate unless kept quite dry.

With a slit $\frac{1}{1000}$ inch in width, and an exposure of 40 minutes, results have been obtained in the region of Great A of the 2nd order which possess all the detail and definition usually so characteristic of the violet end. Numerous lines are sharply depicted which were previously not known to exist. λ 8400 has been reached, giving almost equal detail.*

The process for the preparation of pure cœrulin S is a slight modification of the preceding. The results obtained, as well as the actinic curve, are almost identical. The pure substance is almost white; dilute solutions pass rapidly from pale yellow to a bright green; a trace of ammonia produces an olive-green. In general a solution of aurantia as an absorbent in quartz cell was used.

For several samples of paste I am indebted to the kindness of Messrs. Schott, Segner, and Co., of Manchester, agents to the Badische Anilin- und Soda-Fabrik, Ludwigshafen, who hold the patent rights for the manufacture of alizarin-blue S. It is hoped this company may be induced to manufacture this substance free from the minute crystallisable impurities which render it unsuitable for use in investigations of such delicate nature.

* P.S.—With a low sun at times screens were found unnecessary. Colonel Waterhouse, who has also employed alizarin-blue ('Photographic News,' Oct. 4, 1889), states that Schiendl and Eder failed to recognise the sensitiveness of this substance to the red, and considers that red or yellow screens are required to produce the full effect.

IV. "On Certain Properties of Metals considered in Relation to the Periodic Law." By W. C. ROBERTS-AUSTEN, C.B., F.R.S. Received March 12, 1891.

In a previous paper published in the 'Philosophical Transactions' (1888, A, pp. 339—349), the effect of about 0·2 per cent. of impurities on the mechanical properties of gold was examined, the results of the experiments showing that metals which diminish its tenacity and extensibility have high atomic volumes, while those which increase these properties have either the same atomic volume as gold or a lower one. The behaviour of aluminium and of lithium appeared to be somewhat exceptional. Gold contaminated with 0·2 per cent. of aluminium should, if the theory set forth in the paper be correct, have a tensile strength of about 7 tons per square inch; but it was found to be capable of sustaining a load of nearly 9 tons per square inch without breaking. It became necessary, therefore, to ascertain whether the cooling of a mass of gold containing aluminium presents any peculiarities, more especially as Osmond's* recent work leads to the conclusion that a pure metal can exist in two distinct molecular forms, and that the passage of the ordinary modification of a metal to the allotropic one may either be hastened or retarded by the presence of impurity.

In order to continue the investigation, a trustworthy pyrometer was needed, and this has fortunately been provided by the thermo-electric junction of platinum, and platinum with 10 per cent. of rhodium, the use of which was suggested by M. Le Chatelier.† It appears to be superior to any other of the thermo-junctions which have previously been used, and some experiments made in 1889 satisfied me that the appliance is an extremely delicate and useful one for temperatures between 500° and 1100° C. In a recent report to the Institution of Mechanical Engineers, in which details of the method of calibration are embodied, I have described a suitable arrangement for obtaining, by the aid of photography, autographic curves which represent the cooling or heating of masses of metal.

It consists in enclosing a galvanometer of the Déprez and d'Arsonval type in a large camera; a fixed mirror, F, being placed below the movable mirror, M, of the galvanometer, so that the light from the lime cylinder, L, reflected in the mirror H, passes to both mirrors, F and M, and is reflected in the direction of a fine hori-

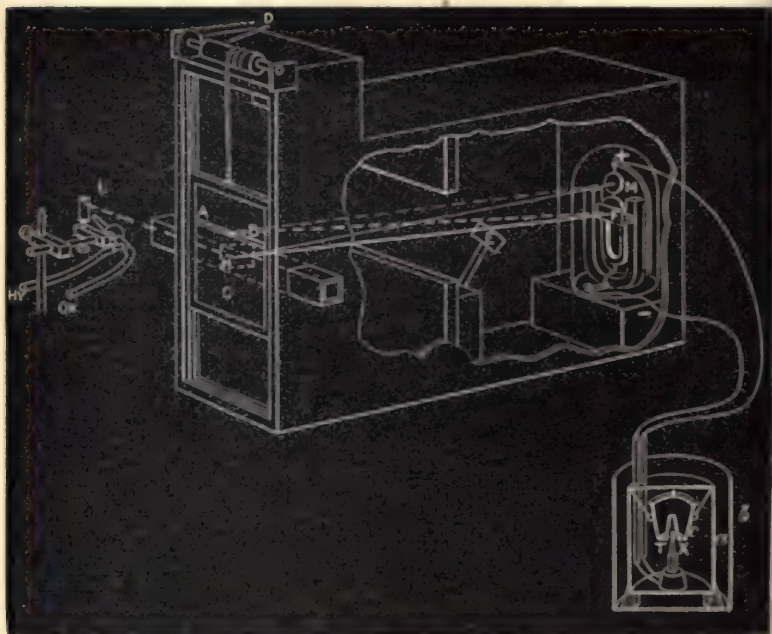
* 'Comptes Rendus,' vol. 110, 1890, p. 346. 'Journ. Iron and Steel Inst.,' 1890, Part 7, p. 38.

† 'Bull. Soc. Chim., Paris,' vol. 47, 1887, p. 2. 'Journal de Physique,' vol. 6, 1887, p. 23.

zontal slit, AB, behind which a sensitised photographic plate, C, is drawn vertically, past the slit, by means of gearing, D, driven by clockwork. The ray from the fixed mirror is interrupted periodically by the vane, E, and a beaded datum line is given which enables any irregularity in the advance of the plate to be detected.

The amount of divergence from its datum line of the spot of light reflected by the movable mirror at any given moment bears a relation (which can readily be found by calibration) to the temperature to which the thermo-junction X is heated, and variations of temperature are recorded by a curve which is the resultant of the upward movement of the plate and the horizontal movement of the spot of light. The complete arrangement is shown in the diagram fig. 1.

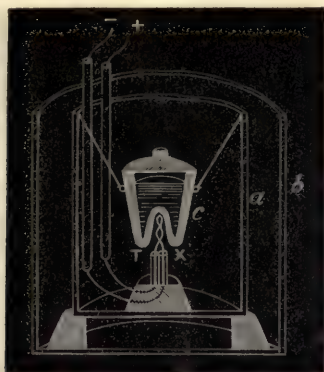
FIG. 1.



The portion of the arrangement in which the thermo-junction is placed is also shown in fig. 2, which is drawn on a larger scale than fig. 1, the same letters being used in both.

The thermo-junction X is inserted in a tubulure, T, of a specially constructed crucible of plumbago, c, which contains about 5 oz. of pure molten gold, and is allowed to cool down slowly inside a vessel, a, of silver 105 mm. diameter, and polished internally. The cylinder, b, is of tin plate, polished internally and blackened outside.

FIG. 2.



A photographic record of the cooling of pure gold is represented by the thicker of the dotted lines in fig. 3. The mass of gold had in this case an initial temperature of about 1250°C ., which fell to 1045°C . when the mass began to solidify. The curve is approximately horizontal during solidification, and throughout its entire course appears to be a perfectly normal curve of a cooling mass of metal, no points of exceptional absorption or evolution of heat, such as would occur in iron, being observable.

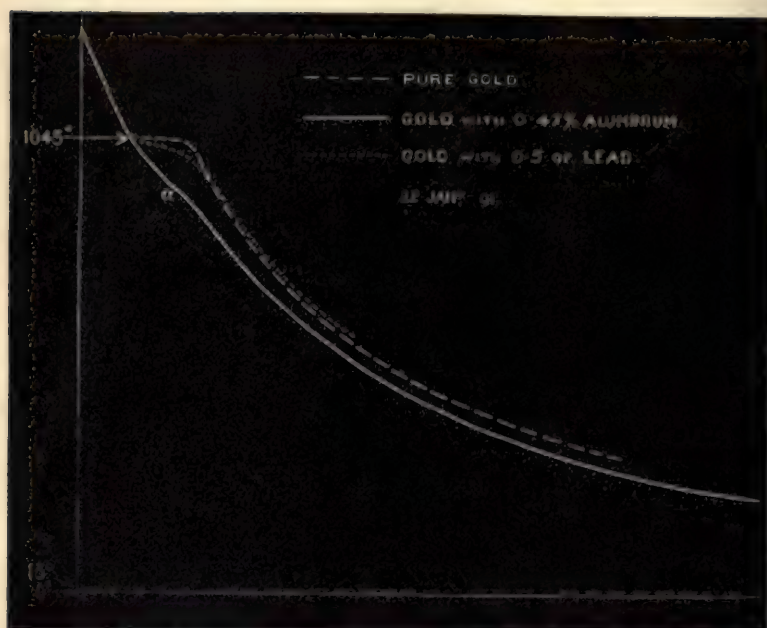
A curve obtained in a similar way, and representing the cooling of gold with 0.5 per cent. of lead, is shown by the thin dotted line in the same figure. It is similar to the one representing the cooling of pure gold, but it will be evident that the presence of lead lowers the freezing point of gold by an amount which is found by measurement to be about 7.5°C .

A very different molecular condition is, however, established by the presence of aluminium. With 0.47 per cent. of this element the true freezing point can be detected, but is nearly obliterated (fig. 3), and the mass does not become truly solid until the point marked *a* is reached when the temperature has fallen to 900°C .

It is of interest to ascertain how far the lowering of the freezing point of gold is in accordance with the results of Raoult's investigations on the lowering of the freezing point of solutions. His generalisations have been tested in the case of solutions of metals in metals with low melting points (tin, lead, and bismuth), in an admirable series of experiments by Heycock and Neville.* In order to calculate the lowering of the freezing point of gold produced by one atom of the added element to 100 atoms of the solvent, which has been the usual method of stating such results, it is necessary to know the latent

* 'Journ. Chem. Soc.,' vol. 55, 1889, p. 666; vol. 57, 1890, pp. 376 and 656.

FIG. 3.



heat of fusion of gold, and this had not been determined, probably because the accurate measurement of the latent and specific heats of metals with high melting points, such as gold, presents many more difficulties than the determinations of similar constants for bodies having low melting points.

Violle* found the specific heat of platinum at different temperatures by heating a piece of the metal in a specially constructed muffle, the temperature being simultaneously determined by means of a porcelain air-thermometer. The temperature of the metal being known, he plunged it into a calorimeter, and calculated from the data he obtained the mean specific heat of platinum between the extreme temperatures of the experiment. By making many experiments at different temperatures, he was able to deduce the specific heat of platinum at any point within the range, and he found that it regularly increased with the temperature. The data, thus afforded, enabled him to obtain the freezing point of platinum by transferring metal just *after* solidification into the calorimeter. Further, by pouring metal just *before* its solidification into water, it was easy to determine the total amount of heat evolved during cooling. Hence, knowing

* 'Comptes Rendus,' vol. 85, 1877, pp. 543—546.

the amount of heat evolved by the mass after it had become solid, and deducting this amount from the total heat transferred to the water by the melted platinum, he obtained the latent heat of fusion of the metal.

He also determined* the specific heat and melting point (1045°C.) in the case of gold, but he does not appear to have ascertained what is the latent heat of fusion of the precious metal. The following experiments were therefore made, in order to afford a basis for calculating the theoretical lowering of the freezing point of gold which a given addition of impurity should produce.

A calorimeter of polished silver, 10.5 cm. diameter, and 15.5 cm. high, was supported upon three points of cork within a bright metallic vessel, blackened externally, and constituting an air-jacket. The amount of water employed varied from 800 to 1088 grams. The stirrer was a thin sheet of mica, mounted upon a silver wire, bent at its lower end into the form of a hoop; the mica also served to catch the gold poured into the water. In experiments E to I, the stirring was effected by silver vanes, of a form suggested by Professor Rücker, actuated by a small electromotor. Quantities of very pure gold, varying from 68 to 123 grams, were melted in a small clay crucible and poured into the calorimeter.

This portion of the manipulation was performed by my assistant, Mr. Groves, whose long experience in melting gold enabled him to select the latest moment before solidification at which the gold could be poured. In Experiments A and B and E to I, the temperature of the molten mass was measured, by the aid of the thermo-junction previously described, which was placed directly in the molten gold, up to the moment of pouring, and it is believed that the temperature of the mass was known to within 10°C.

At the end of each experiment, the gold poured into the calorimeter was carefully collected and weighed. The thermometer used was a very sensitive one with fixed zero, and made by Hicks, of Hatton Garden; it could easily be read to 0.02°C. The depth of water employed was sufficient to prevent the evolution of steam, and none was observed to escape in any of the experiments recorded.

The calculations are as follows:—

Let P = weight of water in calorimeter.

p_1c_1 = water equivalent of calorimeter.

p_2c_2 = " " thermometer.

p = weight of gold employed.

c = average specific heat of gold (Violle).

t = initial temperature of the molten gold.

T = " " water.

t_1 = final temperature of the water.

* 'Comptes Rendus,' vol. 89, 1879, pp. 702—703; 92, 1881, pp. 886—8.

The total quantity of heat carried to the calorimeter by p grams of gold consists of the amount of heat which was required to raise the temperature of the metal to its melting point, *plus* the amount actually required to melt it; or

$$(P + p_1c_1 + p_2c_2)(t_1 - T) = pc(t - t_1) + p\lambda,$$

where λ is the latent heat of fusion of gold required;

$$\therefore \lambda = \frac{(P + p_1c_1 + p_2c_2)(t_1 - T) - pc(t - t_1)}{p}.$$

Taking, therefore, 16.3 as the latent heat of fusion of gold, and proceeding to find the lowering $\delta\theta$ of the freezing point, due to the presence of an impurity,

$$\delta\theta = \frac{\theta w}{\lambda \rho},$$

where

θ = freezing point of gold, from absolute zero,
 w = osmotic pressure in dynes,
 ρ = density of the solvent,
 λ = latent heat of fusion of the solvent in dynes.

Inserting values

$$\begin{aligned} \delta\theta &= \frac{\frac{1040 + 273}{1} \times \frac{(1013 \times 10^3 \times 22.3 \times 1300)}{19.6 \times 273} \rho}{\rho \times 16.3 \times 41.6 \times 10^6} \\ &= \frac{1313 \times 1013 \times 22.3 \times 1300}{19.6 \times 16.3 \times 41.6 \times 10^3 \times 273} \\ &= 10^{\circ}6 \text{ C.} \end{aligned}$$

Experiments are in progress with a view to ascertain whether the mean specific heat of very pure gold is the same as that found by Violle, for there is every reason to believe that the presence of impurity has great influence upon this constant. A few measurements already made would seem to indicate that his result is low, and this is important, because a slight difference in the specific heat will have a material effect upon the latent heat of fusion, and consequently on the theoretical atomic fall in the freezing point produced by aluminium. In order to ascertain whether aluminium would give the normal lowering of the freezing point ($10^{\circ}6$ for each atom present in 100 atoms of gold) a crucible, fitted with a tubulure, as already described, was taken, and 130 grams of very pure gold was melted in it. The crucible was then placed over the thermo-junction, and allowed to cool, the freezing point of the metal it contained

Table of Experiments. In Centigrade-gramme Units.

	P.	p_{1c_1}	p_{2c_2}	p .	c .	t .	T.	t_1 .	λ .	Remarks.
A ...	800.75	15.42	0.86	84.72	0.0352 Violle	1075**	13.4	19.0	16.83	Solidification had just begun.
B ...	800.75	15.42	0.86	68.68		1050*	12.9	17.4	17.07	
C...	933.1	15.72	1.16	87.11		1045	9.68	14.52	16.51	
D ...	808.7	15.72	1.16	76.05		1045	10.34	15.18	16.29	
E ...	1088.6	16.37	1.16	119.6		1045*	11.79	17.47	16.36	
F....	1088.6	16.37	1.16	123.0		1045*	11.74	17.50	15.62	
G ...	1088.6	16.37	1.16	79.19		1045*	12.11	15.88	16.43	
H ...	1088.6	16.37	1.16	105.05		1085*	13.52	18.61	16.05	
I....	1088.6	16.37	1.16	102.75		1070*	14.30	19.21	15.87	
										16.83. Mean of the nine experiments.

* These temperatures were measured by the aid of the pyrometer, and, as the metal was slightly above its melting point, it was necessary to assume that the specific heat of the fluid gold is practically the same as that of solid gold near its melting point. The slope of the autographic curves, moreover, shows that any error so introduced would be but small. [*April* 20, 1891.—Experiments E to I have been added since the paper was read.]

being recorded in the usual way by a curve. The gold was then re-melted, and a weighed quantity of aluminium added, the mass being stirred and the temperature of its freezing point measured and recorded in a curve.

The addition of 0.2 per cent. of aluminium produced an appreciable fall in the freezing point, but this initial fall is only indicated by a change in direction of the curve. The fall as measured upon the photographic plate is only 1 mm., which nevertheless corresponds to a difference of temperature of $7^{\circ}68$ C.

A further addition of 0.2 per cent. of aluminium (making 0.4 per cent.) increased the fall to 1.8 mm., corresponding to $14^{\circ}28$ C. It may be urged that these measurements are small, but the observations were repeated with a scale some distance from the galvanometer, and chronographic records gave results having the same values.

An experiment with gold in which 1 per cent. of aluminium was present also confirms this; the fall in temperature of the freezing point was in this case $33^{\circ}66$ C., but there were indications that the gold, the solvent, was becoming saturated.

Now 0.2 per cent. of aluminium corresponds to $2/10 \times 196/27.5 = 1.42$ atom per 100 atoms of gold.*

Hence the fall per atom present per hundred atoms of gold = $7.68/1.42 = 5^{\circ}4$ C.

Similarly, a percentage of 0.4 corresponds to $4/10 \times 196/27.5 = 2.85$ atoms per hundred of gold, and the fall per atom = $14.28/2.85 = 5^{\circ}0$ C.

With 1 per cent. of aluminium the total fall will be due to 7.16 atoms per hundred of gold; hence the atomic fall will be $33.66/7.16 = 4^{\circ}7$ C.

It may be added that experiments (as yet incomplete) seem to show that lead, bismuth, silicon, and platinum cause a much greater "atomic fall" in the freezing point of gold than aluminium does.

The relations of aluminium to gold would, therefore, appear to be peculiar in more ways than one. The curve (fig. 3) clearly indicates that aluminium has a remarkable influence on the cooling of a mass of gold, and in view of this it would seem strange that calculations based on the atomic weight of aluminium should show that it delays the *initial* solidification of gold less than other elements. The *complete* solidification is, however, much retarded; for merely stirring a mass of gold contaminated with very little aluminium reveals the fact that the added element has set up during the solidification of the mass a "pasty stage" which continues through an unusually long range of temperature.

In the metallurgy of iron, aluminium is known to play an im-

* 196 is the atomic weight of gold, 27.5 that of aluminium.

portant part, and the introduction of a small quantity of it renders it possible to cast very mild steel or even "wrought" iron into forms which are remarkable for delicacy and soundness. The mode of action of the aluminium on iron in the "mitis castings" has given rise to wide divergence of opinion, but the view that it acts by the removal of oxide, or of occluded oxygen, has gained much favour. In the case of gold, which has neither occluded oxygen nor oxide to lose, the castings of the metal with 0.2 per cent. of aluminium are also remarkably sound, and, as experiments prove, very tenacious; the action of the aluminium is, therefore, probably a molecular one of much complexity.

It may be pointed out that the presence in gold of quantities of silver which vary from 0.1 to 4.0 per cent. does not lower the freezing point of the mass. Messrs. Heycock and Neville, who witnessed certain of the experiments above described, inform me of the hitherto unpublished fact, observed by them, that the presence of thallium does not lower the freezing point of lead.

The close concordance in both these cases between the atomic volumes of the mass of metal and the added impurity is of special interest in connexion with the generalisation given in my earlier paper and re-stated on the first page of this. Silver has the same atomic volume as gold, and if present in small quantity, produces no effect on either its tenacity or its freezing point.

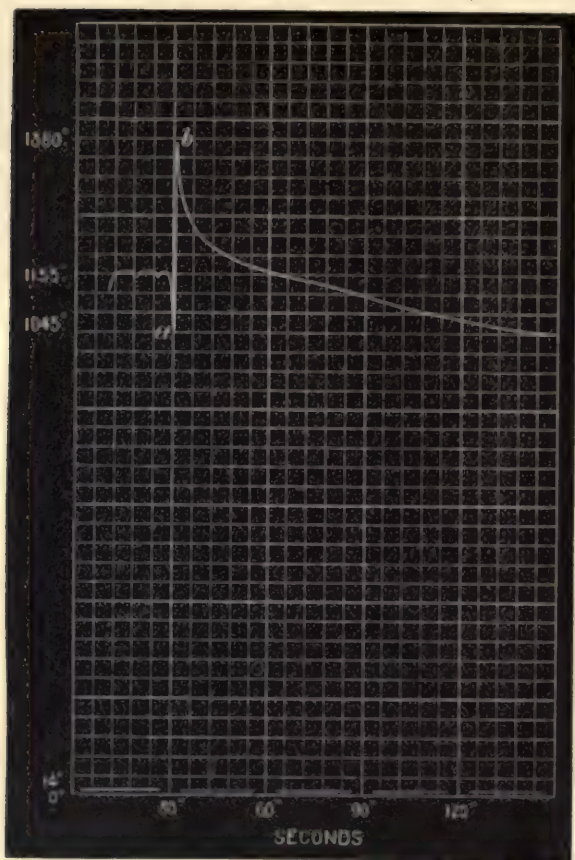
Throughout these experiments gold has simply been employed for the sake of its freedom from liability to oxidation, but other metals must be studied, and it is worthy of record that Hadfield has recently shown that the parts played by aluminium and by silicon in steel are almost identical. Most of the physical properties of aluminium and silicon, in a free state, are totally different, but they possess the same atomic volume, and when they are alloyed with iron they affect it in precisely the same way.

I have to express my thanks to my assistant, Mr. H. C. Jenkins, for his aid in conducting these experiments.

[April 20, 1891.—In the course of the investigation, it became evident that, as is the case when aluminium is alloyed with copper or iron, the addition of aluminium to gold is attended with evolution of heat. The following experiment was therefore arranged, with a view to obtain evidence on this point:—

A mass of 30 grams of gold, contained in an unglazed porcelain crucible, was placed in the centre of a block of firebrick and strongly heated up to well above the melting point of the metal. The thermojunction was inserted directly in the gold, and the spot of light from the galvanometer allowed to fall in the usual way on to the sensitised plate (fig. 1). A piece of cold aluminium, equal in weight to 1 per cent. of the mass of gold, was then added and rapidly stirred. The

FIG. 4.



autographic curve, from which fig. 4 was plotted, showed that the first effect of the added aluminium is, as might be expected, to lower the temperature of the gold to a point *a* which proved to be close to its solidifying point, 1045° ; the temperature instantly rises, however, to a point *b*, which is 225° higher than the initial temperature of the gold. The experiment is not strictly quantitative, as the perfect admixture of the aluminium could not be ensured; but it is probable that true combination of aluminium and gold has taken place, which would doubtless greatly affect the physical constants of the mass.]

Presents, March 12, 1891.

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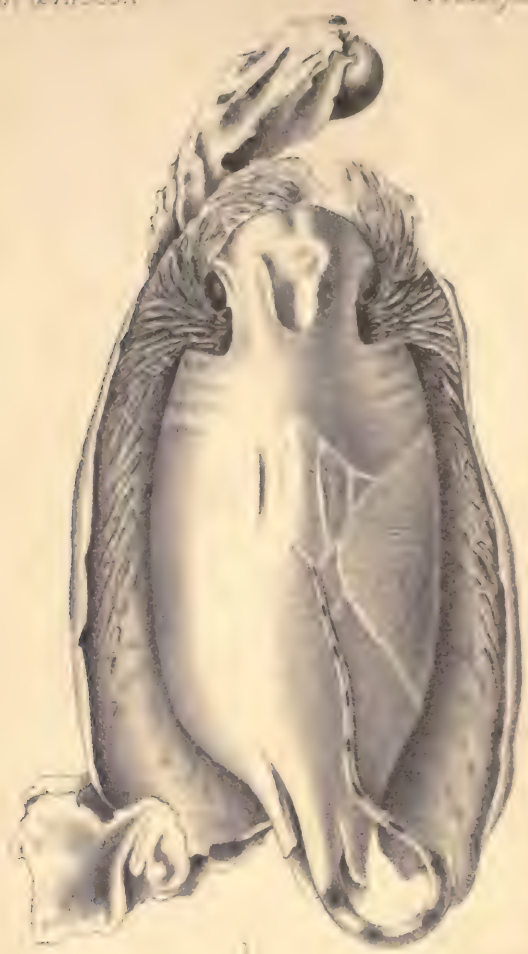
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2

PTEROPLATTA MICRURA

EXPLANATION OF PLATE 7.

Fig. 1. Gravid uterus of *Pteroplatea micrura*, opened by a longitudinal incision in its dorsal wall, with the flaps turned back, so as to show the passage of the large bundles of trophonemata through the spiracles into the pharyngeal cavity of a single foetus.

Fig. 2. Head and shoulders of the same foetus from the left side, to show the great size and the lateral position of the spiracles.

EXPLANATION OF PLATE 8.

- Fig. 3. Distal moiety of a large trophonema from one of the spiracles, to show the superficial texture and the outstanding vein. $\times 14$.
- Fig. 4. Apex of the same trophonema, to show the compound duct-openings. $\times 66$.
- Fig. 5. Transverse section of a trophonema in its distal half, to show the glands in vertical section. $\times 120$. *a*, artery; *V*, main trunk; and *V'*, *V'*, branches of vein with coagula in their lumina.



3 x 14



5 x 120



4 x 66



March 19, 1891.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer and Vice-President,
in the Chair.

The Right Hon. Lord Hannen was admitted into the Society.

The Presents received were laid on the table, and thanks ordered
for them.

The following Papers were read:—

- I. "On the Uterine Villiform Papillæ of *Pteroplatea micrura*,
and their Relation to the Embryo, being Natural History
Notes from H.M. Indian Marine Survey Steamer 'Investigator,'
Commander R. F. Hoskyn, R.N., Commanding. No. 22." By J. WOOD-MASON, Superintendent of the Indian
Museum and Professor of Comparative Anatomy in the
Medical College of Bengal, and A. ALCOCK, M.B., Surgeon,
I.M.S., Surgeon-Naturalist to the Survey. Communicated
by Professor M. FOSTER, Sec. R.S. Received February 24,
1891.

Contents.

- § 1. Preliminary Historical Sketch.
§ 2. Recent Observations on the Uterine Villi of some Indian Rays.
§ 3. The Uterus and Embryo of *Pteroplatea micrura*, and the Relation of the
Uterine Villiform Papillæ (or Trophonemata) to the Embryo.

§ 1. *Preliminary Historical Sketch.*

That in the females of several Selachioids and Batoids the mucous
membrane of the terminal portion of the oviduct, or uterus, is pro-
vided with glandular structures which secrete an albuminous fluid
destined in some way or other for the nourishment of the developing
embryo is a bionomic phenomenon which has attracted the attention
of numerous physiologists and histologists, though, as far as we are
aware, it has never been fully followed out.

Concerning the intra-uterine arrangements for the protection and
nutrition of the egg or embryo among Selachians, Gegenbaur
(*'Grundzüge der Vergleich. Anat.,'* p. 875) writes:—"The terminal
division of the oviduct of the Selachians, which has already been
mentioned as functioning as uteras, also differs from the rest of the

oviduct in the nature of the mucous membrane. In many it is raised into villi. Glands are much developed. The relation of this division of the oviduct to the egg or to the embryo that originates therefrom is a tolerably various one. Least intimate is it in the oviparous *Selachii* (*Raja* and *Scyllium*): only furnishing the egg-case. In others (*Spinax*, *Acanthias*, *Scymnus*), a shell is also developed, but only for a short time, and the embryo afterwards lies free in the uterus. Here come those *Selachians* in which there is no longer any shell at all developed, and from this circumstance results the nutritive connexion of the foetus with the uterine wall by the intermediation of the yolk-sac."

This passage, although it clearly defines the well-known functions of the *Selachian* uterine mucosa, on the one hand of secreting an egg-case, and on the other hand of forming a vascular nutritive connexion with the modified yolk-sac of the embryo, makes only a vague allusion to another modification of function: namely, the secretion by special uterine glands of a nutritive fluid which might be absorbed or ingested by the developing embryo.

Leydig also ('Handbuch der Histologie,' Ed. 1857, p. 517) describes the vascular uterine villi of *Acanthias*, *Spinax*, *Scymnus*, and *Trygon*, though without discussing their relation to the egg or to the embryo. He says:—"In the *Selachians* the mucous membrane (of the uterus) either appears smooth and possessing only zig-zag longitudinal folds (*Scyllium*, for example), or it bears much-developed villi (*Acanthias vulgaris*, *Spinax niger*, *Scymnus lichia*, *Trygon pastinaca*). These villi are placed sometimes (*Acanthias vulgaris*, *Scymnus lichia*) in very regular longitudinal rows which cease towards the end of the uterus, and pass into leaf-like longitudinal folds; or, as in *Trygon pastinaca*, they are so crowded together that nothing more of the rest of the mucous membrane of the uterus appears. The villi possess an exceedingly rich vascular supply: there are to be distinguished in them at most two stronger vessels which interlace and run into one another at the end of the villus, and between the two a close-meshed vascular network. These vessels are in the gravid uterus distinguished by a proportionately very thick circular muscular layer." There are figures (p. 318, Ed. 1857) of "the uterus of *Trygon*, opened, in order to show the villi"; and of "a bit of the uterine mucosa of *Spinax*, somewhat more than the natural size," showing the much-developed villi, which are represented as two- to three-branched.

The great comparative anatomist Johannes Müller, in his exhaustive paper: "Ueber den glatten Hai des Aristoteles" ('Abhand. Ak. Wiss. Berlin,' 1840, p. 188), reviews more completely the state of knowledge up to that date of the *Selachian* uterine structures which, quite apart from the secretion of egg-coverings or the formation of yolk-sac placentæ, are concerned in the elaboration of secretions

which are to be considered either as providing nourishment for the growing embryo after the nutritive yolk of the egg has been absorbed, or as maintaining and increasing the nutritive material of the yolk.

Of *Spinax niger* he says (*loc. cit.*, p. 236):—"The foetus in the uterus is distinguished by possessing no trace of an egg-shell, and by the inner membrane of the uterus being beset by very long (six to eight lines long) filiform villi."

More particularly of the genus *Torpedo* he writes (*loc. cit.*, p. 239):—"The eggs possess no trace of shell membrane; they are only surrounded by an albuminous uterine fluid, as already Redi, Stenonis, Lorenzini, and in more recent times J. Davy, observed. Cavolini states that the yolk sticks to the walls of the uterus, and that this is effected by an innumerable crowd of red uterine glands lying on the yolk. By this are clearly meant the papilliform villi on the uterus of *Torpedo oculata*; but the yolk-sac in no way adheres to the uterus, as the observations of J. Davy show, observations with which what I myself have seen in the gravid uterus sent by Dr. Peters accords. The yolk-sac of the *Torpedos* is perfectly smooth. I can confirm the extraordinary difference observed by T. Davy in the structure of the mucous membrane of the uterus, which in *Torpedo oculata* is furnished with villi, in *Torpedo marmorata* with parallel longitudinal folds." Johannes Müller also quotes Davy's observations on the increase in weight, from the undifferentiated egg-stage to the completed foetal stage, in *Torpedo*, notwithstanding the disappearance of the external yolk and the absence of any vascular maternal connexion.

Of the Spinacoid Shark *Scymnus lichia* he observes (*loc. cit.*, p. 237):—"In the fresh uterus, the foetus and yolk-sac are surrounded by a white-of-egg-like fluid. Also in this Shark no trace of an egg-shell membrane is met with. The foetus with its enormous yolk-sac is immediately surrounded by the uterus. The whole oval yolk-sac is, in the younger and middle stages of development, 4 inches long and 2 inches thick. The inner membrane of the uterus is furnished with cylindrical villi six lines in length."

John Davy ('Phil. Trans.,' 1834, pp. 531—540), in the research on the embryology of *Torpedo*, referred to by Johannes Müller, describes the mucosa of the gravid uterus of this Batoid, and specially dwells upon the absence of any vascular connexion between the embryo and the mother.

He states that, in several series of observations, while the average weight of undifferentiated eggs was 182 grains, and the average weight of eggs in which the embryo had appeared was 177 grains, the average weight of "ripe foetuses" was 479 grains; and he asks how, in the absence of any sort of structural connexion between the foetus and the mother, this remarkable increase of weight is to be explained.

He observed that there was to be found in the uterine cavity "a little fluid, generally milky, more rarely glairy, and sometimes bloody, which, on evaporation, yielded crystals of common salt, and a very little animal matter composed chiefly of albumen;" and he speculated on the possibility of this uterine secretion being nutritive. He negatived, however, the supposition that the embryo *in utero* could take in food by the mouth, and inclined, finally, to the opinion that the embryo increases by absorption, partly through the general surface of the body and partly through the branchial filaments.

Sir Everard Home ('Phil. Trans.,' 1810, p. 208), in a paper on the "Mode of Breeding of the Ovoviviparous Shark," which is quoted by Joh. Müller, described the naked-eye anatomy of the oviduct of *Acanthias vulgaris*, the jelly and which fills that part of its cavity and which functions as a uterus in this Selachian. He states that when the young one is completely formed the yolk remains attached to the belly by a long cord of blood-vessels, and that the young one in this state "swims" in the uterine jelly. The jelly he regards as a secretion of the oviduct.

Home further quotes the observation of Dr. Patrick Russell, that a Shark caught in lat. 70° N. had the oviducts distended with young ones, each with the yolk-sac attached; the young ones swimming in a "white gelatinous liquid, thicker than the liquor amnios of Quadrupeds." Home supposed that the function of the jelly was to aerate the foetal blood.

To sum up, we find that in *Spinax niger*, *Scymnus lichia*, *Acanthias vulgaris*, *Trygon pastinaca*, *Torpedo oculata* (and also in *Myliobatis noctula* and *Centrina salviani*, according to Trois, in a paper in the 'Atti del Istituto Veneto,' vol. 2, which we have unfortunately been unable to obtain), special glandular villi have been observed on the uterine mucosa, which villi have in some cases been supposed to bear, either to the egg or to the developing embryo, some relation of support or nutrition other than that of furnishing an egg-case, or of assisting in the formation of a vascular connexion (yolk-sac placenta) between the embryo and the mother.

In *Scymnus lichia*, on the one hand, it seems certain, from Müller's researches, that the villi secrete a fluid which nourishes the embryo through the medium of the yolk-sac; while in *Torpedo*, on the other hand, it is certain that, at least in the later stages, the secretion reaches the embryo in some less indirect manner, and the question has actually been asked whether it is ingested by the embryo.

In corroborating the opinion as to the function of the villi of the Selachian uterine mucosa, we are able to bring evidence of a case in which the nutritive secretion is actually conveyed into the pharynx of the embryo.

Following on this, and because the term "villus," in its connexions

with mammalian physiology, has come to connote a structure which essentially absorbs nutriment, we propose to term the villiform structures of the uterine mucous membrane in Selachians, which essentially secrete nutriment, *trophonemata*; and by this name they will be referred to in the descriptive portion of this paper.

§ 2. *Recent Observations on the Uterine Villi of some Indian Rays.*

In the 'Journal of the Asiatic Society of Bengal,' vol. 59, Part II, p. 51, the second-named contributor of this paper described some observations on the uteri and the gestation of *Trygon bleekeri* and *Myliobatis nieuhoffi*, which it is necessary here to briefly recapitulate.

In a large female of *Trygon bleekeri*, taken on the 15th December, 1888, in the Máhánaddi estuary, the distal end of the right oviduct was found to be enormously dilated, and to contain in its cavity a fully-developed male foetus with a disk $11\frac{3}{4}$ inches long and $10\frac{3}{4}$ inches broad. The folded foetus lay free in the uterine cavity, quite destitute of membranous covering, without any structural connexion with the mother, and without any vestige of external yolk-sac. The uterine mucous membrane, which was a vivid vascular scarlet, such as in Fishes is usually seen in the gill-laminæ only, was covered with an abundant highly-albuminous fluid, secreted by a crowded layer of glandular filamentous villi which formed the inner coat; and it was inferred that this secretion was a uterine milk elaborated for the nourishment of the embryo. In the absence of any special absorbent organs, and because the embryo was so folded that any supposed "absorbent function" of the skin would have been as much as possible limited, it was further inferred that the "milk" was taken in by the embryo by the mouth; though unfortunately the foetal stomach was not examined until *post-mortem* changes were advanced. It may be mentioned that the foetus did not possess branchial filaments.

Again, in *Myliobatis nieuhoffi*, the structure of the uterine glands has been made out, but we have had no opportunity of ascertaining their relation to the egg or to the embryo. In an adult female, taken off the Godávari Delta on the 31st March, 1889, the left ovary was found to be full of enlarged ova, while the distal end of the oviduct formed a globular swelling with thick, muscular walls, and a mucous membrane thickly beset with long foliaceous villi. The entire surface of the mucous membrane, both villous and inter-villous, was found to consist of a close-set aggregation of tubular glands, most of which were simple follicles resembling the Lieberkühnian follicles of human anatomy, though at the periphery of a villus they are commonly racemose.

We have now, in the case of *Pteroplatea micrura*, discovered

another instance of a Batoid which develops a naked embryo in an uterus with a mucous membrane of a complex structure; and while the histological characters of portions of the uterine mucosa are those of a secretory gland, the disposition of these glandular portions is such as to leave no doubt that the greater part of their secretion is poured into the pharynx of the embryo as it lies in the uterus.

§ 3. *The Uterus and Embryo of Pteroplatæa micrura; and the Relation of the Uterine Villiform Papillæ (Trophonemata) to the Embryo.*

In December, 1889, the detached pregnant uterus, with part of one oviduct and kidney, of a Ray which had been captured in the estuary of the Hooghly was sent to the Indian Museum by Mr. A. J. Milner, of the Bengal Pilot Service.

The embryo, after removal, was identified as *Pteroplatæa micrura* (Bl. Schn.). The measurements of its disk are, length 3·4 inches, breadth 6·5 inches; its caudal spine is not yet developed, and the nasal valves are still separate; its liver is of very large relative size, and the intestine is greatly distended with grumous bile-stained material.

The gravid uterus forms a symmetrical ovoid swelling; dorsally its wall is almost membranous, ventrally it is still thin; laterally, and especially antero-laterally, it is thick and muscular.

Internally, it is lined with mucous membrane, which, dorsally and postero-laterally, is quite smooth.

On the ventral aspect, however, the mucous membrane begins to be extended in the form of short, compressed papillæ, and these, anteriorly and laterally, grow by degrees longer and more numerous, until, at a point in the antero-lateral part of the uterus which coincides with the position of the spiracle of the embryo *in situ*, they form on each side a large bunch of long-compressed villiform trophonemata, the histological structure of which will be presently described.

On opening the pregnant uterus by a dorsal longitudinal incision, the naked embryo is found lying prone, head forwards, and tightly rolled in a right-to-left spiral. Although the embryo is in uniform close contact with the uterine walls, there is no sort of structural connexion between it and the mother, and no trace of any such previous connexion: nor can any vestige of external yolk-sac be observed. But by the rolling up of the embryo the spiracles, which are large patent cavities communicating freely with each other and with the pharynx, and smooth-walled, except for a very faint pectination anteriorly, come to have a lateral position, and deep into them passes, on each side, the lateral bunch of long trophonemata above mentioned, in such a way that the secretion of these richly-glandular

trophonemata can only pass down the pharynx of the embryo. The only other external communications of the pharynx, namely, the mouth and gill-clefts, are firmly closed, the opposite margins of the external gill-clefts being in the most complete apposition. There are, consequently, no branchial filaments.

The trophonemata are narrow, strap-shaped processes of the uterine mucosa which widen very slightly and gradually from their base to about the middle of their length, whence they taper still more gradually to their rounded apex. Those which enter the pharyngeal cavity of the foetus are the longest of all, measuring from 18 to 20 mm. in length and about 1.4 mm. in extreme breadth. Save for the presence of a blunt thickening that traverses them in a somewhat sinuous course from base nearly to apex, tapering and branching as it goes, and standing out in fairly bold relief from both their surfaces, they appear quite flat.

Stained in borax-carminé, mounted in spirit, and viewed under a low power by reflected light, they present a minute, honey-combed appearance of their surfaces due to the presence of innumerable small depressions. From the appressed conjunction of ridges which bound these, a slightly elevated polygonal network results.

Transverse sections of a trophonema shew that these depressions are the funnel-shaped mouths, narrowing below into the lumina, of small, short, bulb-shaped glands of the same simple tubular type as is seen, for example, in the crypts of Lieberkühn, or, better still, in the gastric glands of anthropotomy.

The glands are arranged perpendicular to the surface, side by side, in the substance of the mucosa—into which they are, in fact, so many involutions of the investing epithelium—so that the bases of those of opposite sides are separated from one another only by blood vessels and a very meagre, if any, connective tissue.

Vertical sections of a trophonema in planes parallel to its surface, or, in other words, transverse sections of groups of glands, shew that these are so closely packed as to be polyhedral by appression.

For a certain distance from the base of the trophonemata the depressions are small and simple, but they soon become larger and compound, each being then the common mouth or short duct of a little group of glands, just as in the human stomach, where one meets with simple glands, each with its independent opening, and with groups of glands opening into a common depression of the mucous surface.

The larger (secondary) depressions, it is obvious from an attentive study of the surfaces of several trophonemata, as well as from transverse sections, have resulted from the further depression of the smaller (primary) depressions in groups; and the raised margins of the former result from the coalescence of those portions of the margins

of the latter which are not involved in the general secondary depression.

The epithelium is very thick in the bulbous basal portion of the glands: here its constituent cells are very long, taper from basement membrane to lumen, and are arranged round an axial cell in the form of a cone, of which the outer cells do not extend far up the sides of the gland. Above this the cells become gradually shorter and less oblique, until at or near the mouth they pass into an ordinary columnar epithelium resting perpendicularly on the basal membrane.

The nucleus of the cells is oval, and stains very strongly. It lies excentrically near the base of the cell, which stains only less strongly than the nucleus itself, so as to form a sharply-defined, coloured basal stratum, in which the nuclei are included, throughout the gland. This deeply-stained band is most conspicuous in the basal cells, which are arranged in the form of a cone, and perhaps constitute the chief, if not the only, seat of active secretion.

Not a single cell in any of the sections has been observed to have undergone mucoid degeneration; but many have been noticed to contain minute globules, probably of secretion, not only in the coloured basal portion, but also in the clear and unstained outer part of their protoplasm.

The sections shew coagulated secretion in the mouths and lumina of many of the glands. The glands measure 0.06 to 0.08 in length by 0.04 to 0.07 in breadth. In a trophonema, 12 mm. long by 1.4 mm. in uniform width throughout, taking 0.05 mm. as the average diameter of the glands, we have calculated that no less than 21,280 glands are present—a result which is probably not far from the mark either way. The trophonemata are exceedingly vascular. Two vessels are present in all: an artery, which runs in the substance of one of their margins, and a vein, which takes a sinuous course not quite along the middle, but nearer to the opposite margin, giving off in its course two or three short branches which may or may not anastomose with the main stem. Both vessels taper towards the apex, near to which they are resolved into a capillary plexus, or rather into a system of narrow sinuous cavities which establishes a communication between the two sets of vessels. The wall of these cavities is formed of a delicate, nucleated membrane answering to, and appearing in some part of almost every section to be in actual continuity with, the endothelial lining and subjacent intima of the vein, into which some of the sinuses can actually be observed to open occasionally.

The vein is much larger than the artery: its calibre in that part of its course across which the section has been taken being no less than five times as great. The artery is strongly contracted and empty: the vein, on the other hand, is fully dilated and filled with coagulated blood, even to its lateral branches and the larger of its

affluent sinuses. Hence in lightly-stained trophonemata, examined entire under a lower power, the artery is only just visible by transference, while the vein presents itself as a very distinct, branched, and tapering dark streak.

The connecting system of sinuses occupies the plane between the layer of glands of opposite sides, sending off processes around the base or specially secreting portion of every gland.

The glandular epithelium possesses no connective tissue framework beyond a filmy nucleated gauze, shreds of which can be traced wherever vacuities occur between the bases of adjoining glands, the whole trophonema consisting, as has already been stated, almost entirely of epithelium and blood vessels.

Having already stated our conclusion as to the function of the trophonemata in *Trygon* and *Pteroplatea*, and having described the path taken by the secretion in the latter, it only remains to consider briefly how far other views of the manner of absorption by the embryo of the nutrient uterine milk might apply in the case of these two fishes. Whatever may be the case in the earlier stages, in the later stages, at any rate, branchial filaments do not absorb nutriment from the maternal wall, for branchial filaments do not then exist.

Again, absorption through the general surface of the body of the embryo can hardly be looked upon as the principal channel of supply, because the embryo, at least in its later stages, is so folded or rolled up as to leave as little extent of surface as possible available for absorption.

[*P.S.*, April 8, 1891.—Since the above was written and despatched, we have had the good fortune to obtain from amongst the contents of the fishermen's nets at Cocanada several pregnant females of *Pteroplatea micrura*. The examination of these specimens in the fresh state, while it confirms our description in its principal particulars, renders some modification of it necessary. (1.) The number of young may be as many as three in each uterus, both uteri being developed. (2.) The number of young may be two in each uterus. Our first qualification will therefore be, *Uterus well developed on both sides; each uterus may contain from one to three young ones.* Again, where two and three young ones exist, the trophonemata (which appear to be fairly well developed over the entire mucosa of the uterus) are specially long opposite to the spiracles of the young one, into which they pass. Apparently—and this will be our second qualification—in the early stages of gestation the trophonemata are equally well developed over the entire surface of the uterine mucosa, but by the pressure exerted by the growing embryo they become greatly atrophied, except on those spots where they can pass into the spiracles of the embryo, and here, perhaps by compensation, they become hypertrophied.]

- II. "A New Test for Albumin and other Proteids." By JOHN A. MACWILLIAM, M.D., Professor of the Institutes of Medicine in the University of Aberdeen. Communicated by Sir WM. ROBERTS, F.R.S. Received March 5, 1891.

Salicyl-sulphonic acid is a remarkably powerful precipitant of proteid substances; it is an extremely delicate reagent for the detection of proteids in solution; it acts upon all the classes of proteid bodies.

I shall state the results I have obtained with this reagent under two heads:—

- I. Its action on the various classes of proteids.
- II. Its use as a test for the presence of proteids in urine.

I. *The Action of Salicyl-sulphonic Acid on the various Classes of Proteids.*

In order to obtain the full effect of this reagent, it should be used in *saturated* watery solution, and a drop or two of this solution should be added to a small amount (*e.g.*, 1 or 2 c.c.) of the fluid to be tested, and the test-tube should be shaken so as to mix its contents well. When any considerable amount of proteid is present, a copious white precipitate at once results; with only minute amounts of proteid a cloudiness or opalescence of the fluid is what occurs. This cloudiness or opalescence is uniformly diffused over the fluid. When dealing with traces of proteids it is well to use a control tube containing some of the fluid to be tested, and if dilution has been performed, another control tube with some of the water (or other liquid) used for dilution along with one or two drops of the salicyl-sulphonic acid. The observer then holds the three tubes between him and the light, and looks through them at a dark ground. It is only, however, when dealing with very slight traces of proteids that these precautions are at all necessary.

A. *Native Albumins.*

(a.) *Egg Albumin.*—Upon this proteid salicyl-sulphonic acid acts with much precision. When a solution is obtained by diluting white of egg with water in the proportion of 1 part white of egg in 20 parts of the mixture, the addition of the reagent causes a dense white precipitate to be at once formed. On boiling, the precipitate becomes markedly flocculent. (The solution of white of egg of course contains globulin, but when this is removed by saturation with magnesium sulphate the albuminous filtrate gives the same reaction with salicyl-sulphonic acid as the original fluid.)

When a similar solution is made with white of egg and the proteid removed by thorough saturation with ammonium sulphate (after slight acidulation) and filtration, the proteid-free filtrate gives no precipitate with salicyl-sulphonic acid. But if some of the precipitate thrown down by the ammonium sulphate, after being washed with a saturated solution of the salt, be redissolved in water, the solution of proteid so made gives a copious precipitate when tested as before. (When considerable amounts of ammonium or magnesium sulphate are present, a few crystals may form and sink to the bottom of the tube, apart from the presence of any albumin. This, however, does not at all interfere with the working of the test, as such crystals, floating in the fluid at first and then sinking to the bottom, bear no resemblance to the uniform turbidity or opalescence dependent on the presence of albumin. These salts are easily removed by dialysis.)

Similarly, when the white of egg solution is treated with a large excess of absolute alcohol (after slight acidulation with acetic acid), so as to precipitate all the proteids, and is then filtered, the filtrate shows no precipitate when tested with salicyl-sulphonic acid; removal of the alcohol from the filtrate does not influence the result. On the other hand, the alcoholic precipitate, when (after being washed with absolute alcohol) it is redissolved in water and tested, gives a striking reaction: a large amount of proteid is at once thrown down.

With very dilute solutions made with white of egg, I have compared the delicacy of the action of salicyl-sulphonic acid as a test for proteids with a number of other reagents more or less commonly employed, and I have found the former to be by far the most delicate and precise of all.

The white of egg solution was successively diluted to various degrees—1 part of the white of egg solution (1 in 20) in 100, 200, 300, 400, 600, 620, 900, and 1000 parts of water or of $\frac{3}{4}$ per cent. sodic chloride solution.

With the first degree of dilution (1 part of the 1 in 20 white of egg solution in 100 parts of water or salt solution) the following results were obtained:—

Boiling after faint acidulation with acetic acid = no reaction.

Xantho-proteic test = slight reaction.

The cold nitric acid test (Heller's) = slight reaction.

Mercurio-potassic iodide = haziness of the fluid.

Salicyl-sulphonic acid = marked cloudiness.

With the second degree of dilution (1 part of the white of egg solution in 200 parts of water):—

Boiling after faint acidulation with acetic acid = no reaction.

Xantho-proteic test = no reaction.

Mercurio-potassic iodide = doubtful haze.

Salicyl-sulphonic acid = marked cloudiness.

When the white of egg solution is diluted 400 times:—

Acidulation with acetic acid and heat = no reaction.

Xantho-proteic test = no reaction.

Heller's test = no reaction.

Mercurio-potassic iodide = no reaction.

Salicyl-sulphonic acid = distinct cloudiness.

When the degree of dilution is increased to 600 or 620 times, there is still a distinct reaction with salicyl-sulphonic acid when the test-tube is compared with control tubes containing—(a) the dilute solution alone, and (b) water with salicyl-sulphonic acid.

And even with still higher grades of dilution (900 and 1000 times) there is still an appreciable effect recognisable a little time after the addition of the reagent.

The amount of proteid present in those dilute solutions is exceedingly small.

Taking the percentage of proteid (albumin and globulin) in white of egg as 12.2, the strength of the original white of egg solution (1 in 20) would be less than 1 in 160.

When this solution is diluted 400 times, the proportion of proteid is less than 1 in 64,000; when diluted 620 times, about 1 in 100,000; and when diluted 1000 times, proteid is present only in the very minute amount of 1 in 160,000.

(b.) *Serum Albumin*.—Solutions containing serum albumin were obtained by saturating serum with magnesium sulphate and then filtering so as to remove the globulin; the filtrate contained the serum albumin. This fluid, when tested with salicyl-sulphonic acid, gave an abundant precipitate.

Again, when the serum is deprived of all its proteids by complete saturation with ammonium sulphate and subsequent filtration, it then fails to give the slightest sign of precipitation on the addition of salicyl-sulphonic acid. On the other hand, the proteid precipitate thrown down by ammonium sulphate, when redissolved in water, gives a dense precipitate with salicyl-sulphonic acid.

Similarly, when serum is deprived of its proteids by means of alcohol, the remaining constituents are entirely unable to give the characteristic reaction with salicyl-sulphonic acid.

The delicacy of the action of salicyl-sulphonic acid as a test for minute amounts of the serum proteids is very striking, just as in the case of the egg proteids.

The following reactions will serve as an illustration of this:—

Some ox serum was diluted with $\frac{1}{4}$ per cent. of salt solution to the extent of 1 part of serum in 1000 parts.

With this dilute fluid various tests were tried :—

Salicyl-sulphonic acid = marked opalescence at once.

Boiling after faint acidulation with acetic acid = no reaction.

Heller's test = no reaction at once. Distinct film at junction of the two fluids in a few minutes.

Xantho-proteic test = no appreciable results.

Mercuro-potassic iodide = marked cloudiness.

Saturated salt solution with hydrochloric acid (Roberts' test) = marked cloudiness.

Serum diluted to 1 in 10,000 :—

Salicyl-sulphonic acid = distinct cloudiness (especially after $\frac{1}{2}$ —1 minute), recognisable on comparison with the control tubes in a suitable light.

Boiling after faint acidulation = no reaction.

Xantho-proteic test = no reaction.

Mercuro-potassic iodide = no reaction.

Roberts' test = no reaction.

Copper sulphate and caustic potash (Piotrowski's) = no reaction.

Heller's test = no reaction at the time nor twenty minutes afterwards.

The amount of proteid present in these dilute solutions is, approximately, as follows :—

Taking the percentage of total proteids in ox serum as 7·5,* the serum diluted to the degree of 1 in 1000 would contain less than 1 part of proteid in 13,000; while with the dilution of 1 in 10,000 the amount of proteid would be about 1 in 130,000.

B. *Derived Albumins.*

(a.) *Acid Albumin.*—A solution of acid albumin, obtained from a solution of white of egg by the addition of a few drops of a dilute acid and subsequent warming, gives a copious precipitate on the addition of salicyl-sulphonic acid.

(b.) *Alkali Albumin.*—A solution of this proteid, obtained by treating the white of egg solution with a dilute alkali, also yields an abundant precipitate on being tested with salicyl-sulphonic acid.

C. *Globulin.*

A solution of globulin obtained from blood serum (by precipitating with magnesium sulphate, and subsequently redissolving in dilute salt solution) gives results similar to albumin.

* Hammarsten, "Ueber das Paraglobulin," 'Pflüger's Archiv,' 1878.

And vegetable globulin obtained from flour (by extracting with 10 per cent. salt solution) behaves similarly.

D. *Fibrin.*

Solutions of fibrin, both when a dilute alkali and when 10 per cent. salt solution are used as the solvents, give white precipitates with salicyl-sulphonic acid.

In the case of all the foregoing proteids (A, B, C, and D) the precipitate does not redissolve on heating; on the other hand it becomes markedly flocculent.

E. *Proteoses.*

Primary albumoses (proto-albumose and hetero-albumose) were prepared from Witte's peptone by precipitating them with sodic chloride and (after washing with saturated solution of salt) redissolving the precipitate (containing some salt) by the addition of water. The solution so obtained gave a marked precipitate with salicyl-sulphonic acid; but in this case the precipitate redissolved on heating and reappeared on cooling.

Solutions of secondary albumose (deutero-albumose) gave similar results.

F. *Peptone.*

Solutions of peptone were obtained from albumin artificially digested with pepsin and hydrochloric acid, by saturation with ammonium sulphate and subsequent filtration. The filtrate contained peptone; it gave no precipitate with nitric acid, while it gave the xantho-proteic and the biuret reactions.

On adding a drop of salicyl-sulphonic acid to a small amount of the solution containing peptone, a precipitate was at once formed. This, like the precipitate of albumoses, readily disappeared on heating and reappeared on cooling.

Solutions containing peptone were also prepared by saturating the artificially digested albumin solution with sodio-magnesian sulphate, and similar results were obtained.

Solutions of Witte's peptone were (after being faintly acidulated with acetic acid) saturated with ammonium sulphate in some cases, and with sodio-magnesian sulphate in others. The filtrate contained peptone, the other proteids having been precipitated by saturation with the salts named and removed by filtration. The peptone solution yielded, on being tested with saturated solution of salicyl-sulphonic acid, a reaction similar to that described above, a precipitate which disappears on heating and reappears on cooling.

When the peptone was removed by precipitation with excess of alcohol and filtration, the remaining fluid failed to give the slightest proteid reaction with salicyl-sulphonic acid.

It will be noticed that there is an important difference in the behaviour of proteoses and peptones as compared with the other proteid bodies under the influence of the salicyl-sulphonic acid; the precipitate yielded by the proteoses and peptones clears up on heating, and reappears on cooling, while the precipitate of the other proteids does not clear up on heating, but, on the other hand, becomes markedly flocculent. In this respect the reagent resembles picric acid and mercurio-potassic iodide, and to some extent also nitric acid. It differs from the latter, however, inasmuch as the latter gives no precipitate with peptones.

As regards the nature of the precipitate of egg albumin, or serum albumin, thrown down by salicyl-sulphonic acid, its general appearance might suggest that not only precipitation, but also coagulation, had occurred, as with nitric acid, &c. But the fact that it is soluble on the addition of a sufficiently large amount of a very weak solution of potassic hydrate (0.1 per cent.) or of sodium carbonate (1 per cent.) shows that no coagulation could have taken place. Solution of the precipitate does not take place as long as any acidity remains in the fluid. And when it has been redissolved the addition of a very small amount of a weak acid (nitric, acetic, sulphuric) can again bring about precipitation.

The precipitate of albumin thrown down by salicyl-sulphonic acid is not redissolved by the addition of even considerable amounts of this acid, nor is it dissolved by nitric acid, except when a large amount of the strong acid is added.

When salicyl-sulphonic acid is made to act upon albumin for some time, especially at a high temperature, and the precipitate is then filtered off, the filtrate shows a very marked coloration, varying from a pinkish tint to a bright amethyst. The filter paper commonly shows a staining of the same colour. When the fluid is filtered hot, the filtrate usually shows evidence of containing albumoses; it becomes turbid on cooling, and clears up on heating. The coloration of the filtrate is most marked and pure when the fluid is clear (*e.g.*, when hot); when it is turbid the colour is, to some extent, masked and modified (often to an orange-pink tint) by the presence of the precipitate.

II. *On the use of Salicyl-sulphonic Acid as a Test for Proteids in Urine.*

Salicyl-sulphonic acid gives no precipitate whatever with normal urine.

In the case of albuminous urine, on the other hand, it constitutes an extremely delicate and precise test for the presence of proteids. By its use, coupled with heat, one can, with great facility, recognise very minute amounts of proteid substance, and can discriminate between the so-called "albumin" (albumin and globulin), most commonly found, and the albumoses or peptones, if such are present. This is easy, since the application of heat in the latter case causes the precipitate to disappear—to reappear on cooling; while, in the case of ordinary albuminous urine, the precipitate does not clear up on heating.

The method of performing the test in the case of urine is the same as with other solutions.

A very small amount of urine should be taken in the bottom of an ordinary test-tube (*e.g.*, half an inch, or so), or a very small, narrow test-tube may be used. The acid must be in a thoroughly saturated aqueous solution. A drop of this solution is then added to the urine, and the tube is shaken. When any considerable amount of proteid is present, a copious precipitation immediately occurs; when there is only a minute proportion of proteid, the fluid becomes uniformly opalescent.

The delicacy of the test is shown by the following results, obtained with one of the samples of albuminous urine examined:—

The urine was diluted to 10, 20, 30, 40, 50 times. With the weakest of these fluids (1 in 50), salicyl-sulphonic acid quickly gave a marked opalescence.

Heller's test gave no reaction for a considerable length of time; then it gave a doubtful haziness at the junction of the nitric acid and the urine.

Picric acid (saturated watery solution) = no reaction.

Cupric sulphate and potassic hydrate = „ „

The urine was then diluted twice as much, to 100 times, and still gave, after standing a little, a distinct cloudiness with salicyl-sulphonic acid, specially noticeable when the tube was compared (in a suitable light) with two control tubes, containing respectively (a) some of the dilute urine, and (b) water with a drop or two of salicyl-sulphonic acid.

Even with much greater degrees of dilution, appreciable results were got by means of this test. Without going to the extreme limits of its application, however, I find that in the case of the urine diluted 100 times, the amount of albumin contained must have been exceedingly small. A quantitative determination showed that the amount of urine present in the *undiluted* urine was about 0.1 per cent. Hence the amount present in the urine diluted 100 times must have been about 1 in 100,000.

The opalescence caused by the addition of salicyl-sulphonic acid to

such very dilute albuminous solutions does not clear up on boiling. It remains persistent for days in the cold, the precipitate after a time assuming the form of a marked cloud at the lower part of the test-tube.

The effect of adding salicyl-sulphonic acid to samples of albuminous urine from which the albumin had been removed was tested in many cases, and always with negative results. The albumin was precipitated by means of absolute alcohol (after acidulation, when necessary) or by saturation with ammonium sulphate. The proteid-free filtrate gave not the slightest reaction in any instance when tested with salicyl-sulphonic acid in the usual way.

The characteristic reaction of even minute amounts of albumin in the urine I found to be given, on the addition of salicyl-sulphonic acid, in very numerous and various conditions—in acid, neutral, and alkaline urine; in urines rich in mucin, phosphates, urates, &c.; in urines containing bile, sugar, and other abnormal constituents. My results, obtained from the examination of a large number of samples of urine, have not indicated that the applicability of the test is complicated or interfered with by any of the abnormal constituents present. The urine of persons under the influence of various drugs (*e.g.*, alcohol, quinine, sulphonal, croton-chloral, iodide of potassium, chloroform, salicylate of soda, strophanthus, &c.) has not shown the slightest reaction with salicyl-sulphonic acid when shown to be free from albumin by other tests (after concentration) or when freed from albumin, if such has been present, by means of alcohol or ammonium sulphate.

I have also examined the effect of salicyl-sulphonic acid upon solutions of various substances, many of which give precipitates with certain of the well-known reagents for the detection of albumin in urine—solutions of strychnine, digitalin, morphia, nicotin, chloral hydrate, atropine sulphate, aconitine, ergotin, caffen citrate, strophanthin, sulphonal, gallic acid, quinine, bromide of potassium, copaiba, &c.—and in no case have I seen any reaction at all resembling that yielded by proteids.

The conclusion to which my results up to the present lead is that salicyl-sulphonic acid is probably the most delicate and precise of all known reagents for the detection of proteids in solution.

Note on the Nature of Salicyl-sulphonic Acid.

Salicyl-sulphonic acid is a whitish crystalline substance, readily soluble in water and in alcohol. On slow evaporation of its aqueous solution, it crystallises in long, thin needles.

Its formula and formation are stated in Beilstein's 'Handbuch d. org. Chemie,' 2nd ed., vol. 2, p. 969. (For this reference I am in-

debted to Professor Japp, F.R.S.) The formula is there given as $C_6H_5(OH)(SO_3H)COOH$; and its formation by the action of sulphuric anhydride on salicylic acid (Mendius, 'Ann. Chem. Pharm,' vol. 103, p. 45), or by heating salicylic acid with concentrated sulphuric acid (Remsen, *ibid.*, vol. 179, p. 107). It is said to be very stable, and to undergo no change on heating with nitric acid.

The specimens of salicyl-sulphonic acid which I have used in my experiments were obtained from Messrs. Davidson and Kay, Union Street, Aberdeen.

III. "The Influence of Oxygen on the Formation of Ptomaines."

By WILLIAM HUNTER, M.D., F.R.S.E. Communicated by Professor M. FOSTER, Sec. R.S. Received March 11, 1891.

(Abstract.)

A special interest attaches to the rôle of oxygen in the life-history of bacteria. Very wide differences exist, however, between different groups in respect of its importance. To the great majority a free supply of oxygen is absolutely essential for their proper growth and development; to a small minority the converse applies, growth proceeding best in the absence of oxygen, if indeed it is not entirely prevented by its presence; while, lastly, in the case of an intermediate group it seems almost immaterial whether oxygen be present or not, growth proceeding apparently equally well in both conditions, provided that the supply of food be otherwise suitable.

Of these three groups of "obligate aërobic," "obligate anaërobic" and "facultative aërobic" bacteria, respectively, the last has perhaps the greatest interest for the pathologist, as it is to it that the great majority of pathogenic organisms belong.

The question is thus an interesting one, to what extent the pathogenic properties of this class of bacteria are related to the power they, apparently under necessity, possess of obtaining their supply of oxygen from the food constituents themselves when the supply in the air is cut off.

The present paper deals with the results of an investigation undertaken in this relation.

It was necessary that the class of bacteria selected for study should be one whose pathogenic properties were not constant, but subject to variations presumably connected with the character of their surroundings.

The bacteria of ordinary putrefaction possess in a special degree this qualification, their chemical products differing much in character and poisonous action under different, for the most part as yet unknown, conditions.

The method chosen by which to gauge the influence of oxygen on the pathogenic properties of bacteria was to estimate the quantity of alkaloidal bodies or "ptomaines" formed in the putrefactive process, according as oxygen (1) was freely admitted; (2) was present in moderate quantity; or (3) was withheld altogether.

For our knowledge of the ptomaines of putrefaction we are chiefly indebted to the researches of Brieger. In their order of formation as well as complexity, the most commonly met are *choline*, $C_5H_{15}NO_2$; *cadaverine*, $C_5H_{14}N_2$; *putrescine*, $C_4H_{12}N_2$; *trimethylamine*, $(CH_3)_3N$; *dimethylamine*, $(CH_3)_2NH$; and *methylamine*, $(CH_3)NH_2$.

The ptomaines most characteristic of the early stages of putrefaction are the *diamines*, which include, in addition to cadaverine (pentamethylenediamine) and putrescine (tetramethylenediamine), other two isomeric with the former, but of different, as yet unknown, constitution—*neuridine* ($C_5H_{14}N_2$) and *saprine* ($C_5H_{14}N_2$).

With the exception of choline, all these bodies are non-poisonous; and choline only produces symptoms when given in very large doses.

In this respect they differ from another group which possess markedly toxic properties, *e.g.*, *muscarine* ($C_5H_{15}NO_3$), an oxidised derivative of choline, and *neurine* ($C_6H_{13}NO$), also obtainable from choline artificially by warming with baryta water; as also two other bodies to which Brieger gave the name of mydatoxine ($C_6H_{13}NO_2$) and *mydine*, $C_8H_{11}NO$.

While the poisonous bases are oxidised, the harmless bases are non-oxidised, a circumstance which led Brieger to conclude that oxygen plays an important part in the formation of poisonous alkaloids, and that a free access of oxygen favours the formation of ptomaines generally.*

The observations now recorded supply data for judging how far these conclusions are correct.

Their chief result is to show that the formation of the ordinary putrefactive ptomaines is favoured by the entire absence of oxygen; the quantity formed under such circumstances being several times greater than when oxygen is admitted.

Method of Research.

The method employed for the isolation of the ptomaines was that of Brieger. Equal quantities of extract of meat, obtained by extracting lean meat with cold water, were allowed to putrefy, for periods ranging from 5 to 8 days, under the three following conditions:—

(1.) *Free Supply of Oxygen.*—The fluid was placed in a large glass cylinder, open at both ends, kept in continuous rotation round a horizontal axis. The direction of rotation was alternately from right

* "Weitere Untersuchungen über Ptomaine." Hirschwald. Berlin, 1885, p. 27.

to left and left to right; the fluid was thus kept in continual agitation, and uniformly distributed over the inner surface of the cylinder.

(2.) *Moderate Supply of Oxygen.*—The fluid was placed in a wide-mouthed jar, and stirred freely from time to time.

(3.) *Exclusion of Oxygen.*—The fluid was placed in a narrow-necked bottle which it nearly filled; excess of oxygen at the outset was driven out by a stream of hydrogen; the bottle was then tightly closed by an india-rubber stopper through which passed a glass tube suitably bent and opening externally under mercury.

In (2) and (3), the vessels were maintained at a uniform temperature of 32° by being suspended in a water tank. In most of the experiments, a certain quantity of extract of pancreas was added, to hasten putrefaction, with 10 grams of CaCO_3 to prevent the injurious action of the acids formed in the early stages of the process.

The conditions of the experiments varied somewhat in other respects, either as regards the quantities of material used, or its nature, or the manner of dealing with it. The experiments made, eight in number, thus divide themselves into three series, each made up of two or three different observations under the conditions above noted.

The attempts made in the earlier experiments to isolate the individual ptomaines in the form of their platinum or gold salts failed, owing to the small quantities present.

Attention was afterwards confined to the diamines, and accurate quantitative results were obtained by use of benzoyl chloride—a reagent which, as Udranzky and Baumann have shown, forms bulky and stable derivatives with all bodies of this nature.

Results.

The results of the observations show:—

(1.) That a free supply of oxygen prevents entirely the formation of ptomaines, the only base found under such circumstances—and that too from the very first—being ammonia.

(2.) With one exception, all the experiments agree in showing that, as judged by the relative quantities of diamines formed, the greatest formation of ptomaines takes place when oxygen is entirely excluded.

The differences between moderate supply of oxygen and entire absence in this respect ranged from 2:1 to as much as 27:1 in the observations made, the greatest formation always taking place where oxygen was excluded.

The one exception to this can be explained by a difference in the procedure, the effect of which was probably to destroy a large number

of the diamines in the observation in which oxygen was excluded. The relation in this instance was reversed, viz., 1:3.8, the largest quantity being obtained where oxygen was admitted in moderate quantity.

(3.) Observations were also made on the effect of lengthening the duration of the putrefactive process when oxygen was entirely excluded. The result showed that on the 13th day the diamines were reduced to one-fourth of the quantity present in a similar amount of fluid, exposed to the same conditions, on the 7th day.

(4.) In all cases the bulk of the benzoyl compound obtained was made up of cadaverine, its melting point varying according to purity from $127^{\circ}5$ C. to 129° C. Putrescine was only present in traces.

(5.) The results of the observations on the quality of the bases present were not so definite. The most definite symptoms of poisoning were obtained in one instance from the injection of a fluid which had putrefied in the absence of oxygen. They included prostration, increased peristalsis, and diarrhœa, and on another occasion rise of temperature.

Conclusions.

Certain conclusions are drawn from the above data, partly of a *special*, partly of a *general*, character.

The results are interpreted as tending to support Pasteur's original views as to the relation of fermentation processes to absence of oxygen, as against those more recently advanced by Schützenberger, Nägeli, Buchner, and others. They show for the putrefactive bacteria at least that a free supply of oxygen prevents fermentation altogether, as judged alike by absence of aromatic products and ptomaines, and by presence of ammonia from the very first.

They also show that Brieger's view before mentioned as to the necessity of the oxygen for the formation of ptomaines must be considerably qualified, the results obtained being entirely opposed to such a view.

As regards the influence of oxygen on the *quality* of the ptomaines formed, the conclusion is drawn that the presence or absence of oxygen is not the chief factor in determining the formation of poisonous, as distinguished from harmless, ptomaines; and that other factors, such as duration of putrefactive process and nature of material, are likewise incapable of doing so.

Both the formation and the character of poisonous ptomaines must be referred to individual characters of the bacteria present, probably also to the influence of "mixed infection," rather than to the physical conditions under which they act, important as the present observations prove certain of the latter to be in modifying in a very material way the fermentative action of bacteria generally.

IV. "Some Measures of Young's Modulus for Crystals, &c."

By A. MALLOCK. Communicated by LORD RAYLEIGH, Sec. R.S. Received March 9, 1891.

The Table at the end of this communication contains the results of experiments made in October, 1889—February, 1890, on the elasticity of various bodies.

The measures relating to crystalline bodies are, I believe, new.

The method used to obtain these results was applicable to very small specimens. This was a necessary condition in the case of most crystals, because of the difficulty of getting large specimens without flaws.

In the experiments now to be described, I am dealing only with the values of Young's modulus, but by a modification of the apparatus measures can be made of the simple rigidity, which will, I hope, form the subject of a future communication.

Of course, the simple rigidity must lie between one-half and one-third of the value of Young's modulus, according to the ratio between longitudinal extension and lateral contraction (Poisson's constant) for the substance.

The apparatus used in my experiments is shown in figs. 1 and 2.

Fig. 1 shows the general arrangement of the parts, and fig. 2 is a full-sized diagram of the mirrors and knife-edges.

The lettering is the same in both figures. A, is a vertical brass

FIG. 1.

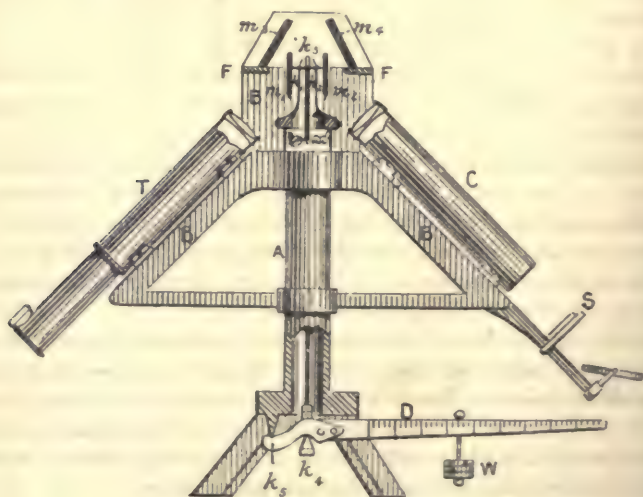
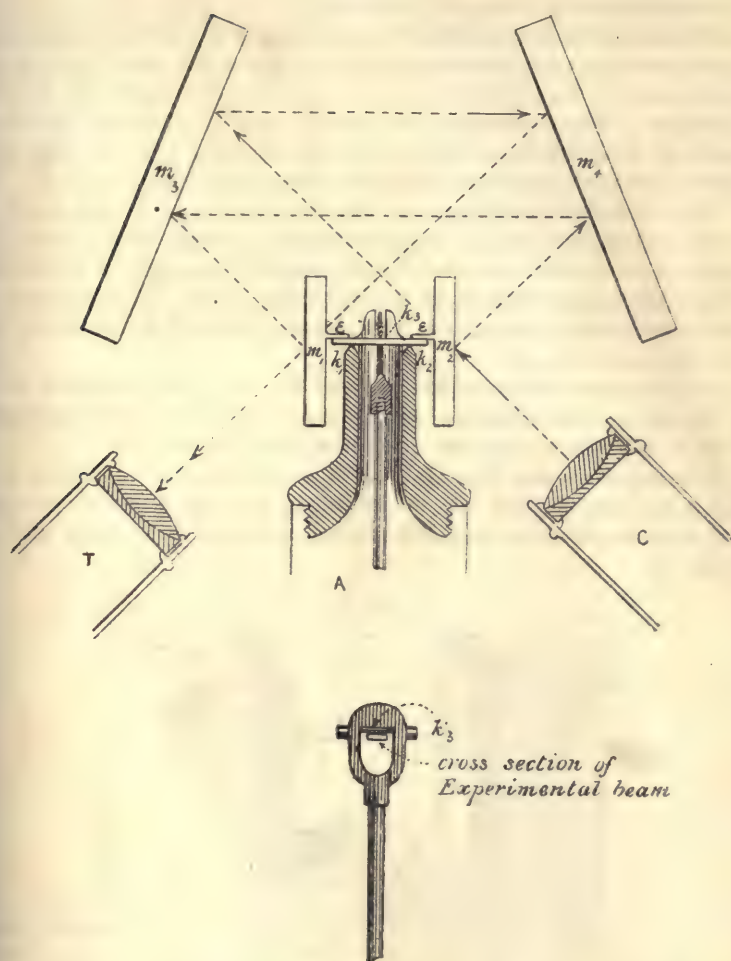


FIG. 2.



tube carrying the oblique arms B, B'. On B is mounted the telescope T, and on B' the collimator C, having in its principal focus the glass scale S.

K_1 and K_2 are two parallel horizontal knife edges, mounted on a brass support at the upper end of A. On these knife edges the substance to be examined rests, and a third knife edge, K_3 , parallel to the other two, and half-way between them, which is properly guided and free to move only in the vertical plane passing through its edge, presses on the substance with a force determined by the magnitude of the weight W and its position on the graduated arm D.

The fulcrum of D is the knife-edge K_3 , and a wire passing through A connects the knife-edge K_1 with K_3 .

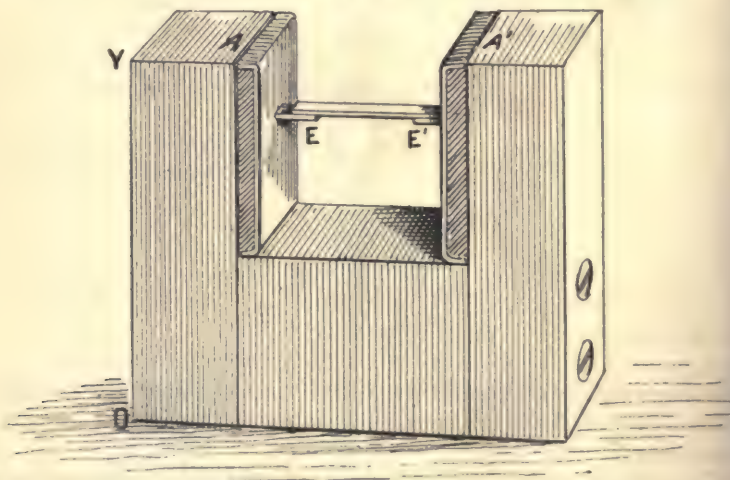
The substance to be examined is formed into a small rectangular beam, rather longer than the distance between the fixed knife-edges K_1 , K_2 , and to the projecting ends of the beam the mirrors M_1 , M_2 are cemented. These mirrors are mounted in brass frames, and from the back of each frame a small brass tongue, EE' (fig. 2), projects, which is the actual part to which the cement is applied.

The two other larger mirrors M_3 , M_4 are inclined to one another at an angle of 45° nearly. They are fixed in a rigid brass mounting, which rests on the horizontal flat surface FF , from which two studs project, so placed that when the mounting of M_3 , M_4 is in contact with both, the intersection of the planes of M_1 and M_2 is parallel to the knife-edges K_1 , K_2 .

The horizontal width of the mirrors M_1 , M_2 is less than half that of M_3 , M_4 , and the telescope and collimator are so placed that their axes of collimation graze the vertical edges of M_1 and M_2 .

It is necessary that the planes of M_1 and M_2 should be nearly, but not quite, parallel, and this is effected by cementing the mirrors to the experimental beam whilst the former are held in the gauge shown in fig. 3.

FIG. 3.



A spring, not shown in the figure, keeps the mirrors pressed against the plane faces A, A' of the gauge. These are parallel in the vertical direction but inclined to one another about $2'$ or $3'$ horizontally, i.e., the planes A, A' intersect at this angle in a line parallel to OY.

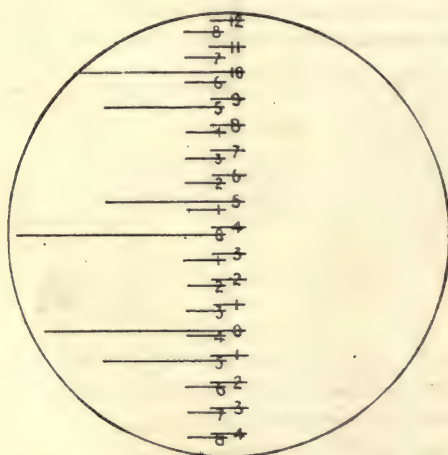
The cement is applied to E and E' before the mirrors are placed in the gauge; when they are in position, the beam is laid with one end on E and the other on E', and warmed. When cool, the experimental beam with the mirrors attached is removed from the gauge and laid in the proper position on K_1 , K_2 .

On looking through the telescope T, two images of the scales S will be seen side by side. One of these images being formed by successive reflection from the four mirrors M_2 , M_4 , M_3 , M_1 , in the order named, and the other by reflection from M_3 and M_4 only.

The course followed by the two sets of rays is indicated by the arrows on the dotted lines in fig. 2.

The appearance of the scales in the field of the telescope is shown in fig. 4. If M_1 and M_2 were absolutely parallel in a horizontal direc-

FIG. 4.



tion, the scale images would at times exactly overlap one another, which would make readings difficult.

As long as the condition of approximate parallelism of the intersections of the pairs of mirrors is fulfilled, any shifting of one image of the scale past the other is due to an alteration of the angle between one pair or other of the mirrors, and to that alone, and since M_3 , M_4 are in a rigid mounting, any relative motion of the images is due to an alteration in the angle between M_1 and M_2 .

The experiments were made by noting the relative motion of the images when force was applied to the beam by the central knife-edge K_3 . The course usually followed was to start with no load on K_3 , and having noted the relative position of the scale images, to move W along D until the scales had moved relatively through one division.

The reading was then taken on D. W being then moved forwards until another scale division had been reached, the position of W on D was again read, and so on. The process was afterwards reversed and readings of the same kind taken when the load was being diminished step by step.

All these results were then plotted, and the curve drawn through the observations gave a measure of the angle between the ends of the beam in terms of the force applied to its middle point. In nearly all cases the lines drawn through the plotted observations were straight lines, within the limits of errors of observation, but, in general, the lines for each substance differed appreciably, according to whether the strains were increasing or decreasing. Some of the plotted diagrams are appended to show the kind of accuracy attained.

The actual linear motion of the central knife-edge was always very small, not in any case exceeding 0.00016 inch.

I pass now to the treatment applied to the experimental results, in order to deduce from them the values of Young's modulus.

If l, b, t are the length, breadth, and thickness of a beam (originally straight),

q = Young's modulus,

F = Normal force applied at its mid-length,

θ = Angle made by the tangent at each end with the tangent at its mid-length,

$$\theta = \frac{3}{4} \frac{Fl^2}{qbt^3} \quad \text{and} \quad q = \frac{3Fl^2}{4\theta bt^3}.$$

If, now, δ be the distance between the divisions of the scale S, n , the number of divisions through which the images of the scales are relatively displaced, and f the focal length of the collimator, the angle observed, $\phi = n\delta/f$.

Now $\phi = 4\theta$, because 2θ is the actual alteration of angle between M_1 and M_2 , and this is multiplied by two by the reflection.

Also, if R be the reading of the position of W on the arm D, and r the distance between K_1 and K_3 , the downward force acting on K_3 is

$$F = W \frac{R}{r};$$

hence

$$q = \frac{3WRf l^2}{n\delta r b t^3}.$$

In this expression, for the value of q , the factor $f l^2 / \delta r$ is a constant, depending only on the apparatus, since l is the distance between K_1 and K_2 .

$1/bt^3$ is a constant for each beam, and R/n is the inclination of the straight line passing through the plotted observations to the axis of n .

Hence putting A for $\frac{3fb^2}{\delta r}$, B for $\frac{1}{bt^3}$, and C for $W \frac{R}{n}$,

$$\text{Log } q = \log A + \log B + \log C.$$

The numerical values of quantities involved in A were as follows:—

Focal length of collimator, $f = 8.87$ inches.

Length between knife-edges, $K_1, K_2, l = 0.3422$ inch.

Distance between division of scale $\delta = 0.01$.

Distance between knife-edges $K_4, K_5 = 1.0$.

The values employed for b and t varied between 0.1 and 0.01 inch, and for W from 0.02 to 0.25 pound. The chief and indeed the only considerable source of error in these experiments is in the measurement of t . The measures were made with a screw micrometer reading to 0.0001 by estimation.

The average value of t was between 0.03 and 0.04, so that the measurement was probably accurate to something like 1 in 400. Hence, there may be an error in t^3 approaching 1 per cent.

In the case of crystalline substances, beams cut from the same neighbourhood of the same crystals exhibit a constancy, in the results obtained from them, of this order, but in passing to other specimens more difference was observed.

In many substances, and notably in the case of zinc, lead, and white marble, it was found that the full deflection due to a given load was not reached until a considerable time had elapsed, and experiments with such substances would of course lead to different values for Young's modulus, according as the observations were made in rapid succession or slowly.

The behaviour of zinc in this respect was so marked, that a separate set of observations were made with that material, the results of which are shown in diagrams (10) and (11), pp. 394–395.

It will be seen on examining these diagrams that, starting with a freshly annealed piece of rolled zinc (and similar results were obtained from a beam cut from a large crystal of cast zinc), that, on the first application of the force, the bending immediately produced continues to increase for many minutes, and that, when the load is removed, the beam does not recover itself all at once, and also that a permanent set has taken place.

On the second application of the force, however, if the force is not greater than that first applied, the behaviour of the zinc is quite different. It now very rapidly assumes its maximum deflection and

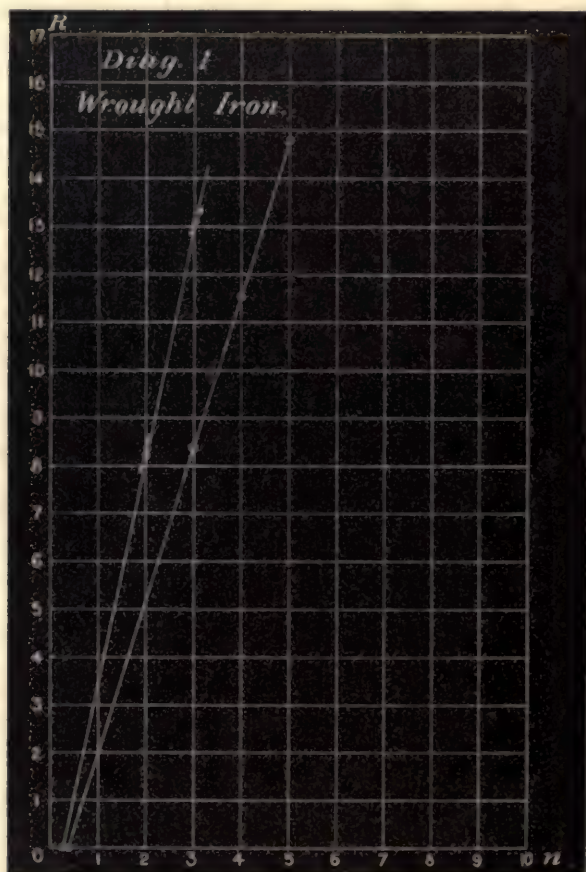
drops back to its equilibrium position, on the removal of the force, still more rapidly, but little further permanent set being produced.

On again increasing the force so as to exceed that first applied, a further gradual extension, lasting a considerable time, takes place; and additional permanent set is found when the force is removed.

Diagram (11) shows that—

- (1.) The immediate elastic bending and the permanent set are proportional in amount to the force causing them.
- (2.) That the increment of extension or deflection which happens in time is something like a constant quantity.

The method, however, described in this paper is not well adapted to the investigation of these phenomena, as the state of strain in the beam on which they depend varies from + through 0 to - on

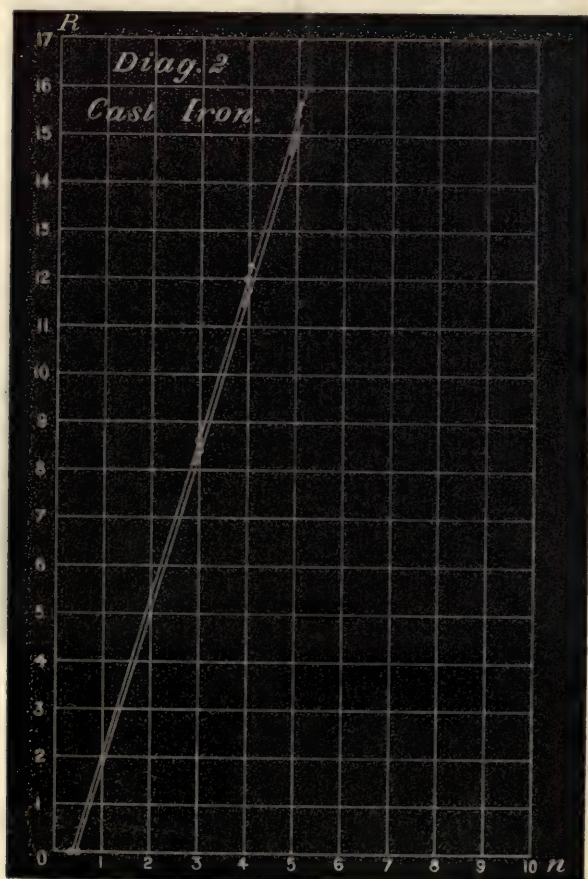


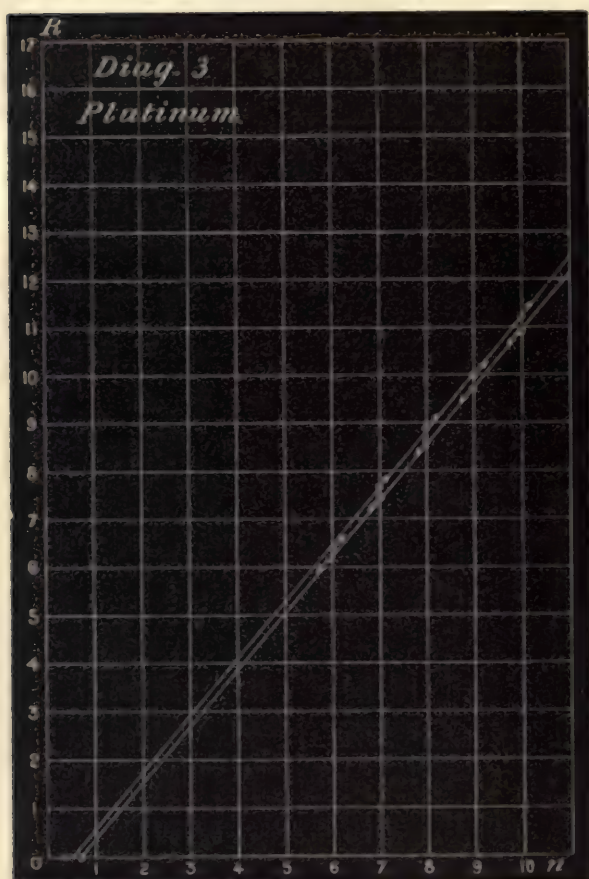
opposite sides of the neutral axis, and one cannot be sure that the very slightly strained material of the central parts acts in the same way as that near the upper and lower boundaries.

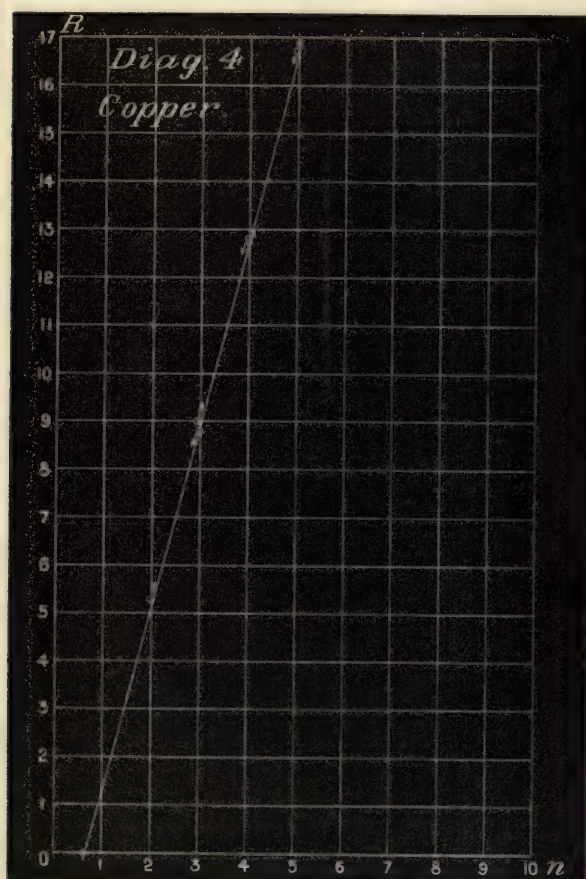
In nearly all the substances experimented on, it was found that work was done in bending and unbending the beams, *i.e.*, for a given deflection the load was always less when the latter was being diminished than when it was being increased. This effect was generally more apparent in metals than in hard crystals.

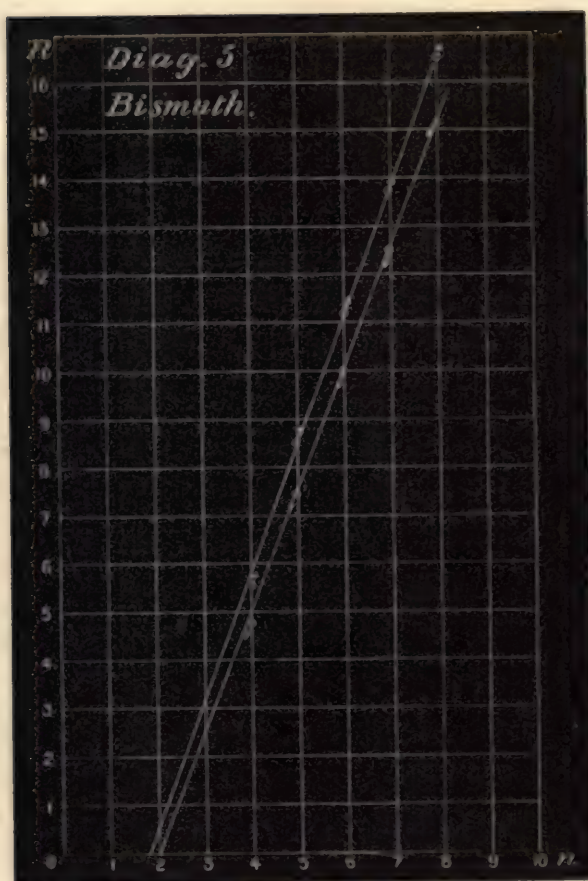
On reference to the Table (p. 398), it will be seen that only ten of the non-metallic substances examined at all approach steel in stiffness.

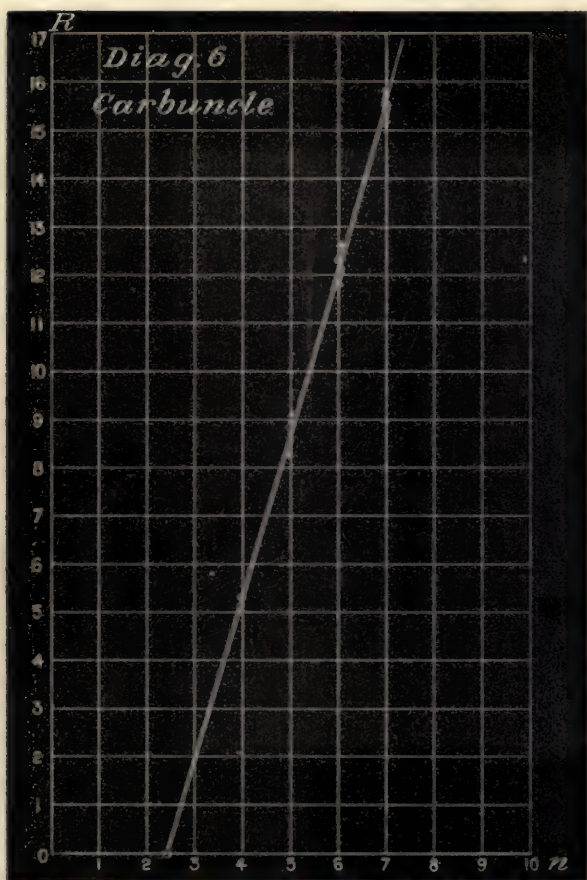
I regret that I have not hitherto been able to get a specimen of diamond of suitable form for measurement; but I hope to be able to give Young's modulus for this and some other crystals in a supplementary table.

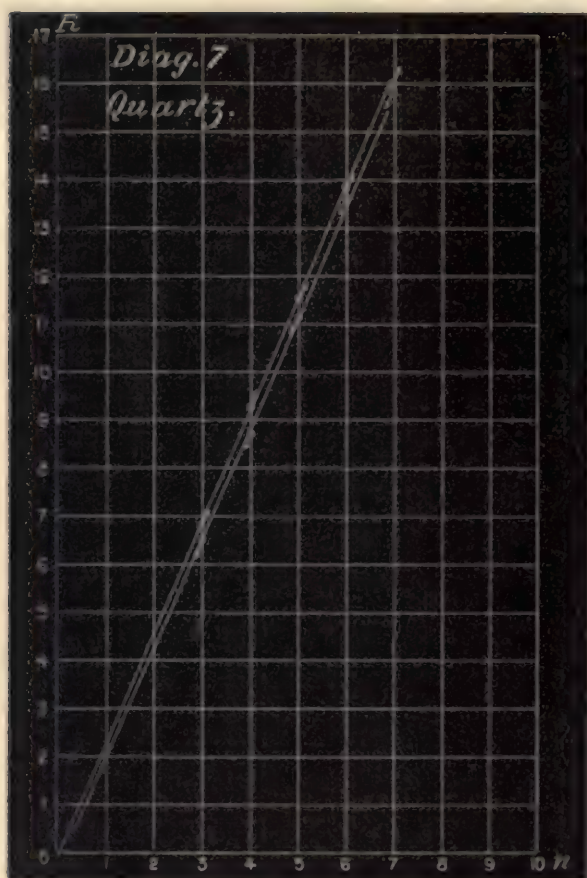


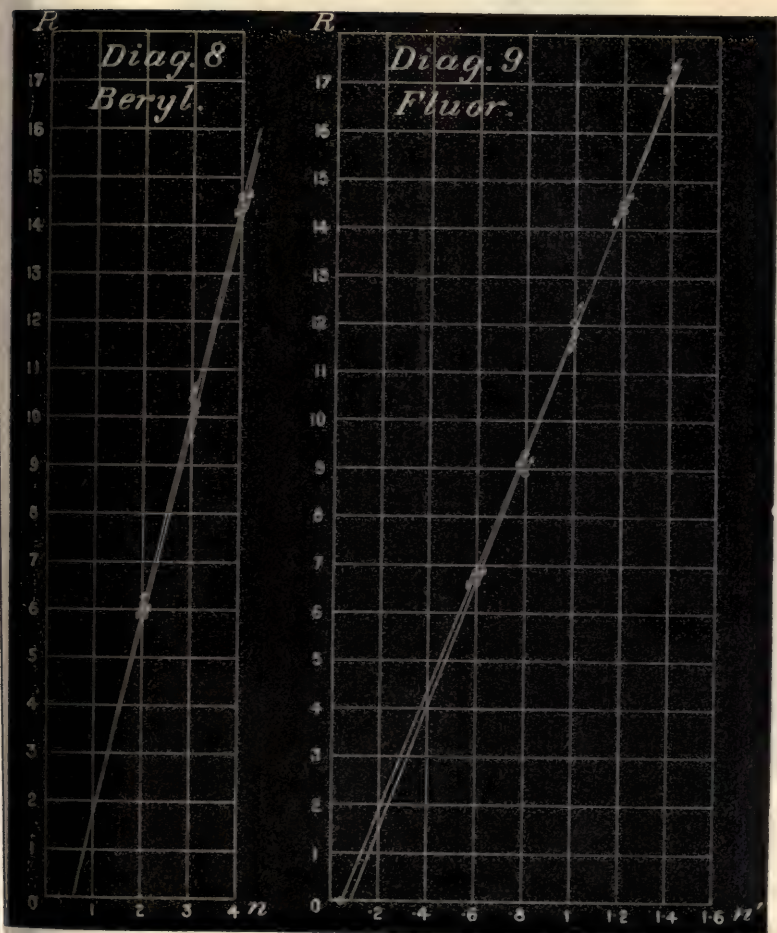


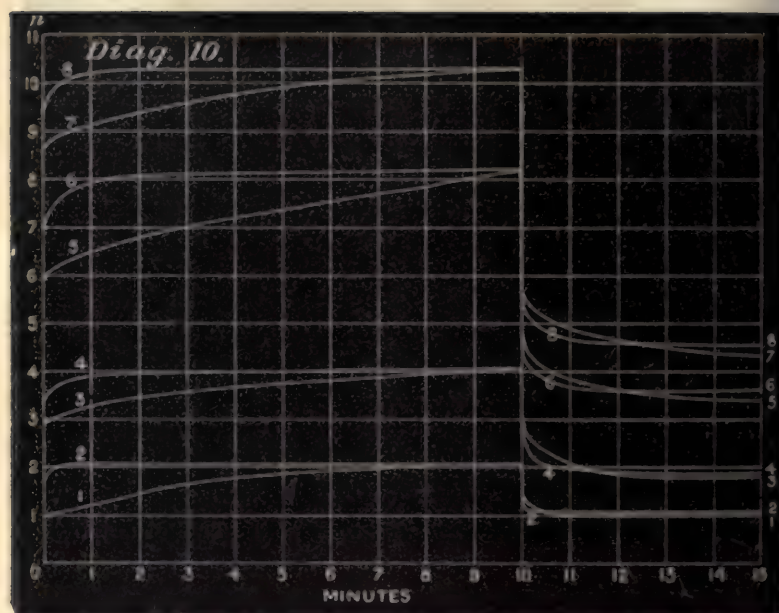


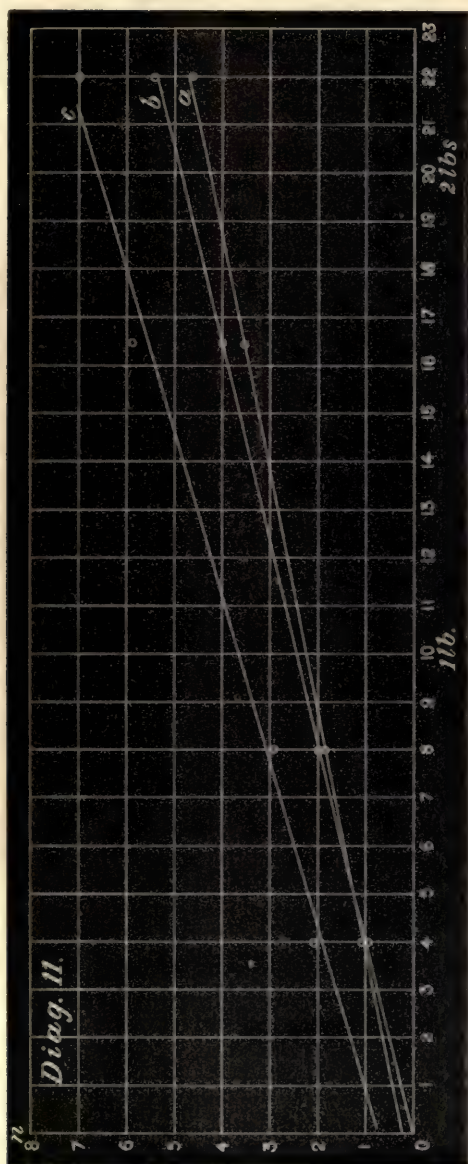












Explanation of the Diagrams.

Diagrams (1) to (9) are examples of the diagrams used in determining the ratio R/n .

The ordinate is the scale-reading on the arm D (fig. 1), and the abscissa the corresponding scale-reading of the images of the glass scale seen in the field of the telescope.

That the line through the observations does not in general point to the origin is due to the fact that the mirrors were not quite parallel in a vertical direction at the beginning of the experiment; in fact, according to the position in which the spring holding the mirrors against the faces of the gauge was placed while they were being cemented to the beam, variations of rather more than a minute of arc were produced between their planes. The abscissa reading when the ordinate = 0 has of course to be subtracted from n in getting the true ratio R/n .

The spots indicate individual observations, and are marked with arrows whose directions show whether the load was being increased or diminished.

Diagram (1). $t = 0.0302$ inch } wrought iron.
 $b = 0.1100$ " }
 (a) $W = 0.2581$ lb. (b) $W = 0.3710$ lb.

Diagram (2). $t = 0.0322$ inch }
 $b = 0.0918$ " } cast iron.
 $W = 0.2511$ lb. }

Diagram (3). $t = 0.0208$ inch }
 $b = 0.1075$ " } platinum.
 $W = 0.0671$ lb. }

Diagram (4). $t = 0.0461$ inch }
 $b = 0.0955$ " } copper.
 $W = 0.1381$ lb. }

Diagram (5). $t = 0.0473$ inch }
 $b = 0.1190$ " } bismuth.
 $W = 0.0671$ lb. }

Diagram (6). $t = 0.0304$ inch }
 $b = 0.0757$ " } carbuncle
 $W = 0.0671$ lb. }

Diagram (7). $t = 0.0369$ inch }
 $b = 0.0911$ " } quartz.
 $W = 0.0671$ lb. }

Diagram (8). $t = 0.0415$ inch
 $b = 0.0877$ „
 $W = 0.1381$ lb. } beryl.

Diagram (9). $t = 0.0292$ inch
 $b = 0.1065$ „
 $W = 0.0217$ lb. } fluor.

In diagram (9) the abscissa is plotted to a different scale, since, owing to the extreme brittleness of fluor spar, a displacement of only about 1.5 of the ordinary division of the scale could be safely use when the thickness of the specimen was about 0.03 inch, and many beams of this substance were broken in attempting to produce greater flexures.

In this experiment the greatest departure of the centre of the beam from its unstrained position is about 0.00001 inch.

Diagram (10) shows some of the properties of zinc. The curves numbered 1 to 8 are really one continuous experiment. A light weight was allowed to act on a beam of sheet zinc, and the deflections caused by it were noted every thirty seconds for ten minutes. These deflections are shown by the first part of curve 1. The weight was then removed, and the recovery of the beam was observed for five minutes. This forms the second part of curve 1.

The experiment was then repeated with the same weight, and the results are shown in curve 2. Additional weight being applied, the same course of procedure gave curves 3 and 4, and in like manner by still further additions of load curves 5, 6 and 7, 8 were obtained.

The dimensions of the beam were :—

$$t = 0.0373 \text{ inch}$$

$$b = 0.1558 \text{ „}$$

For curves, 1, 2	weight = 0.4026 lbs.
„ 3, 4	„ = 0.8052 „
„ 5, 6	„ = 1.657 „
„ 7, 8	„ = 2.110 „

Diagram (11) gives (a) the permanent set, (b) the immediate elastic deflection, (c) the deflection at the end of ten minutes. These are taken from diagram (10).

Table of Values of Young's Modulus.

Substance.	Young's modulus, lbs. per square inch.	Young's modulus. C.G.S.
Steel	33.5 × 10 ⁶	2.311 × 10 ¹²
Wrought iron	27.0	1.863
Platinum	25.42	1.754
Cast iron (soft grey)	23.31	1.608
Copper	17.65	1.218
Brass	16.38	1.130
Cobalt	12.89	8.895 × 10 ¹¹
Aluminium.....	8.73	6.025
Bismuth*	4.16	2.87
Lead	2.71	1.87
Zinc†	1.4 to 0.89	9.7 to 6.1 × 10 ¹⁰
Carbuncle‡	34.83 × 10 ⁶	2.430 × 10 ¹²
Carbuncle‡ (another specimen)	34.38	2.372
Beryl‡	30.9	2.076
Tourmaline§ (a)	18.76	1.294
Smoky topaz‡ (a)	17.5	1.207
Fluor 	17.39	1.200
Fluor¶	17.18	1.185
Yellow topaz‡	16.38	1.130
Yellow topaz‡	13.79	9.515 × 10 ¹¹
Yellow topaz‡ (b).....	12.75	8.80
Tourmaline§ (b).....	11.79	8.135
Quartz**	10.82	7.46
Hard white glass.....	10.09	6.96
Agate.....	9.25	6.381
White "Arkansas" stone††	8.45	5.83
Selenite‡‡	7.98	5.505
"Extra dense" flint glass..	7.48	5.165
Bluish marble	4.64	3.20
White marble	1.6	1.1

* Cast bismuth. The beam cut parallel to a natural crystalline cleavage of the metal.

† The greatest value is that obtained from observations taken in rapid succession.

‡ Relation of the faces of the beam to the crystallographic axes not known. The specimens marked (b) are cut at right angles to those marked (a).

§ A very black opaque crystal from the Ural; (a) cut parallel to the side of the prism, (b) normal to the sides.

|| Parallel to diagonal of the cubic crystal.

¶ Parallel to face of cube.

** Parallel to sides of prism.

†† A very close-grained oilstone.

‡‡ Parallel to the principal cleavage.

V. "On the Chief Line in the Spectrum of the Nebulæ." By JAMES E. KEELER, Astronomer of the Lick Observatory. Communicated by WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. Received March 13, 1891.

As my paper on the Motions of the Planetary Nebulæ in the Line of Sight* did not give a final determination of the exact position of the chief nebular line, and might therefore possibly be regarded as leaving in abeyance the question as to whether that line could be regarded as a remnant of the magnesium fluting, I beg to be allowed to state briefly the results of some more recent observations, which have enabled me to fix with great accuracy the true position of the chief nebular line.

At the time when my paper on the motions of the nebulæ was printed, I had not been able to obtain any satisfactory comparisons of the third nebular line with terrestrial hydrogen, all the nebulæ in my list having proved to be too faint for the purpose. I was, therefore, compelled to adopt the mean position of the principal line for the ten nebulæ observed as the normal position from which to measure displacements, and it was for the reason that the ten nebulæ did not have the uniform distribution in the sky which was desirable that the numerical results for their motions were stated as "not to be regarded as final."

In October, 1890, when the Orion nebula came within reach of the telescope, comparisons of the third line with the $H\beta$ line of hydrogen were made without difficulty, and on the same nights the position of the principal line was determined. One such double observation, if perfect, completely solves the problem, since the displacement of the third line gives the necessary correction to the position of the first. The only question is in regard to the accuracy of the observations.

It is evident from what has already been written on this subject by Dr. and Mrs. Huggins, Professor Lockyer, and myself, that the answer to the question whether the chief nebular line is coincident with the edge of the magnesium fluting at λ 5006.4 depends upon very small differences of position, differences which would, in fact, be considered small even in solar spectroscopy. But their minuteness, although it increases the practical difficulty of observation, does not detract from their importance, since absolute coincidence of spectral lines is necessary (although not always sufficient) to establish a claim to identity of origin. It is therefore necessary to determine from a careful consideration of the Lick Observatory measures whether they are of a sufficiently high

* 'Publications of the Astronomical Society of the Pacific,' No. 11, p. 265.

order of accuracy to prove that the small observed interval between the nebular line and the magnesium fluting is real, and not due to errors of observation.

A detailed account of all the tests to which the apparatus was subjected cannot be given here. Nothing that suggested itself was omitted. The best tests, however, both for constant and for accidental errors, are afforded by observations of the motion in the line of sight of bodies whose motion is already known. As an example of such observations, I may refer to the measures of the motion of Venus in the line of sight given in the table on p. 270, 'Publications of the Astronomical Society of the Pacific,' No. 11, in which the greatest error is one English mile per second. Similar measures of the displacement of lines in the lunar spectrum were seldom in error by more than two miles, and measures of the motion of α -Tauri and α -Orionis, usually made on the same nights that the nebula was observed, were of the same order of accuracy, as determined by their agreement with each other, and with the photographic results of Professor Vogel.

In work of this character the periodic shifting of lines in the spectra of the stars and nebulae due to the earth's annual motion is of a magnitude not to be neglected, and it should appear in the comparison of observations made at different seasons. So faithfully is the orbital motion of the earth reflected in my observations on the nebula of Orion, that I would with some confidence undertake to determine the month of the year, by measuring the distance of the principal line from the lead line used in the comparison spectrum.

With these remarks on the degree of accuracy which characterises the observations, I give below the results which have been obtained, up to the present time, for the nebula of Orion.

From sixteen complete measures, made on eleven different nights (two of which were in the winter of 1889-90), the wave-length of the principal line, corrected for orbital motion of the earth, is λ 5006.22 $\pm$ 0.014, the probable error corresponding to an uncertainty of 0.5 mile per second in the line of sight. When two measures were made on the same night, they were always in different spectra of the grating.

Ten comparisons of the third nebular line with terrestrial hydrogen were made on seven nights in 1890-91, showing, when corrected for the orbital motion of the earth, a displacement of the nebular line toward the red of 0.28 ± 0.026 tenth-metres. This corresponds to a motion of recession of the nebula from the sun of 10.7 ± 1.0 miles per second.

In recent comparisons of hydrogen with the third nebular line, I have not been able to attain the small probable error of $1\frac{1}{2}$ miles per second for a single evening's comparison, given in my letter to the

'Observatory,' as the first comparisons were made under exceptionally favourable conditions. Some small improvements in the apparatus make it probable, however, that it can be reached in the future.

Examination of the individual results for each night's work shows that the errors are purely accidental; hence, the mean of the results for the third line will be used to determine a correction to the mean of the results for the first line.

A displacement of the third line toward the red of 0.28 tenth-metre corresponds to a displacement of the principal line, in the same direction, of 0.29 tenth-metre, which is the amount by which the principal line is seen to be too near the red end of the spectrum, on account of the recession of the nebula from the sun.

Hence the wave-length of the principal line, if determined by an observer at rest relatively to the nebula, would be λ 5005.93, and this, therefore, is the *normal position* of the chief nebular line, according to all the observations of the nebula of Orion which have been made, up to the present time, at the Lick Observatory. The probable error of this result is, by the theory of least squares, 0.03 tenth-metre. The position of the MgO fluting, on the same scale, is λ 5006.36 or 0.43 tenth-metre below the normal position of the nebular line. An interval of this magnitude is not only measurable with my apparatus, but noticeable at a glance in the telescope.

An incident which occurred during the course of the work may be mentioned here, as showing how much greater the above-stated interval is than any error which could be made under good conditions of observation. The measures of January 26, 1891, on being reduced the next morning, made the interval between the nebular and lead lines 0.15 tenth-metre greater than it should have been according to previous measures. This difference led me at once to infer that something was wrong with the apparatus, and on examining the instrument I found that the observing telescope was set to a reading 5° different from the usual one, in such a direction that a higher dispersion than usual had been employed. On determining the value of the micrometer for this position of the grating, and re-reducing the observations, the discrepancy was then but a few hundredths of a tenth-metre.

In the 'Journal of the British Astronomical Association,' Mr. Maunder says, in reference to the possibility of my having over-measured the interval between the chief nebular line and the edge of the magnesium fluting, "Further, some allowance must be made for the difficulty of comparing a line with a fluting; we ought certainly not to measure from the centre of the nebular line to the extreme edge of the fluting. This will apply a small, but a further, correction in the same direction." Mr. Maunder's criticism does not, however,

apply to my own observations, which were made with this difficulty in view. If the distance between the line and the edge of the fluting could be measured with a slit-width vanishingly small, the true interval would be obtained. With a practicable slit-width, the position of the centre of the line is unchanged, but the edge of the fluting is shifted toward the red by half the width of the line. In my observations of nebulæ, the slit-width used was such as to make the bright, sharp lead line (and hence, also, the nebular line) just the width of the coarse micrometer wire (about 0.4 tenth-metre). The bright lines were observed by occulting them with the wire, the observations thus referring to their centres, but the magnesium fluting was observed by bringing its extreme edge and the lower edge of the micrometer wire into coincidence, the centre of the wire falling therefore upon the edge of the fluting with infinitely narrow slit. Measures of the interval between the lead line and the edge of the magnesium fluting, made with the fine micrometer wire and as narrow a slit as could be used, gave the same value as measures made in the manner just described.* The correction mentioned by Mr. Maunder is therefore unnecessary.

It appears to me, from what has been shown above, that the non-coincidence of the chief nebular line and the magnesium fluting must be regarded as proved.

In regard to the character of the line, recent observations at Mount Hamilton have shown nothing which does not confirm the opinion I have already expressed,† that under no circumstances of observation does the line tend to assume the aspect of the remnant of a fluting.

The observations which have been made at Mount Hamilton demonstrate the incorrectness of the view that the chief nebular line is in any way connected with the magnesium fluting at λ 5006.36, for reasons which may be briefly summarised as follows:—

(1). The nebular line is 0.43 tenth-metre more refrangible than the lower edge of the magnesium fluting.

(2). The nebular line has no resemblance to a fluting.

(3). Flutings and lines of magnesium, which could not fail to

* I may call attention to the fact that my own value of this interval (1.86 tenth-metres) is 0.04 tenth-metre *smaller* than the most reliable measures which have yet been published.

† "A single prism of 60° was first employed, then a compound prism of about three and one-half times the dispersion of the latter, and finally a Rowland grating of 14,438 lines to the inch. With all these different degrees of dispersion, and also with other spectroscopes employed, the nebular lines appeared to be perfect monochromatic images of the slit, widening when the slit was widened and narrowing to excessively fine, sharp lines when it was closed up. The brightest line showed no tendency to assume the aspect of a 'remnant of a fluting' under any circumstances of observation."—'Publications of the Astronomical Society of the Pacific,' No. 11, p. 266 and 280.

appear at the same time with the fluting at λ 5006,36, are entirely absent in nebular spectra.

Additional reasons have been given by Professors Liveing and Dewar, and by others who have investigated the subject, but I wish to consider here only such observations as have been made at the Lick Observatory.

The Society then adjourned over the Easter Recess to Thursday, April 9th.

Presents, March 19, 1891.

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April 9, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, followed by the Treasurer, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

- I. "On Electrostatic Screening by Gratings, Nets, or Perforated Sheets of Conducting Material." By Sir WILLIAM THOMSON, D.C.L., P.R.S. Received April 2, 1891.

I. Grating.

1. Maxwell, in his "Theory of a Grating of Parallel Wires" ('Electricity and Magnetism,' Arts. 203—205,* and Plate XIII), gives a very valuable and interesting two-dimensional investigation of electrostatic screening, and a most instructive diagram of "Lines of Force near a Grating," which powerfully helps to understand and extend the theory, and to acquit it of an accusation wrongly made against it in the last two sentences of Art. 205. It is only on the supposition of the grate-bars being circular cylinders that the investigation is less than rigorous: and that supposition nowhere enters into the investigation; it merely appears in the word "radius," in the first line of the last sentence but one of Art. 204, and it is contradicted in lines 3 and 4, and by the rest of the sentence, and by the next sentence. (See § 6 below.)

2. The conclusion, " $\alpha = -0.11a$," in the last sentence of Art. 205, condemned as "evidently erroneous," is quite correct, and very interesting. It shows that a corrugated metal plate agreeing with

* In formula (7) of Art. 204, delete λ ; in Art. 204, delete 2 in last line of p. 250 (Edition 1873); and delete 2 in lines 6 and 16 from foot of the page.

the equipotential surface $c = \frac{1}{2}a$, exceeds in electrostatic capacity a plane metal surface through the poles of the diagram (Plate XIII, reproduced in § 9 below), with the surroundings described in Art. 204, and supplies the datum requisite for finding the exact amount of the excess. The reason for the greatness of the excess clearly is that the surface $c = \frac{1}{2}a$, which just touches the plane through the poles of the diagram midway between the poles, is everywhere nearer than this plane to the other plate of the condenser. (See § 7 below.)

3. For $c = a/6$ we have, by (11) of Art. 205, $\alpha = 0$, and the corresponding equipotential, partially shown in Maxwell's diagram, is a set of curves concave towards $z = -\infty$, and asymptotic to the lines $x = (i \pm \frac{1}{4})a$, i denoting any integer. (See §§ 10 to 13 below.) For every value of c less than $a/6$, the equipotential is a row of ovals; and the grating formed by constructing these ovals in metal has less electrostatic capacity in the circumstances described in Art. 205 than a plane through the poles or the ovals (this being no doubt what is meant by "a plane . . . in the same position" as the grating).

4. For every value of c exceeding $a/6$ the equipotential, instead of being the boundary of a grating, is a continuous corrugated surface, and its electrostatic capacity exceeds that of the plane through the poles.

5. Begin now afresh, and let it be required to find the electric force in the air on either side of an infinite row of parallel bars at equal consecutive distances, a , each uniformly charged with electricity. Let ρa be the quantity per unit length on each bar, so that ρ would be the surface density, if the same quantity were uniformly distributed over the plane of the bars. Taking O in one of the bars, OX perpendicular to the bars, and OZ perpendicular to their plane, we find (by Fourier's method) for the z -component of force at any point (x, z) for which z is positive,

$$Z = \frac{4\pi\rho}{a} \left(\frac{1}{2} + \epsilon^{-ms} \cos mz + \epsilon^{-2ms} \cos 2mz + \&c. \right) \dots \dots (1),$$

where

$$m = 2\pi/a \dots \dots \dots (2).$$

Summing this we find

$$Z = \frac{2\pi\rho}{a} \frac{\epsilon^{ms} - \epsilon^{-ms}}{\epsilon^{ms} - 2 \cos mx + \epsilon^{-ms}} \dots \dots \dots (3).$$

This has equal positive and negative values for equal positive and negative values of z , and it therefore gives the value of the z -force, not only for positive, but also for negative, values of z . Taking now $-\int Z dz$, with constant assigned to make the integral zero for $z = \pm D$, we find

$$V = \rho a \left(\log \frac{1}{\epsilon^{ms} - 2 \cos mx + \epsilon^{-ms}} + mD \right) \dots \dots \dots (4)$$

as the potential due to the grating, and two parallel planes at equal distances, D , on its two sides, each uniformly electrified with half the quantity of electricity of opposite sign to that on the grating.

6. If now we construct in metal, C , any one complete equipotential surface, V_0 , of this system, and electrify it with the same quantity of electricity as that which we gave originally to the infinite row of infinitely thin bars; and if we place metal planes, B, B' , at the two places of zero-potential ($z = \pm D$), we have an insulated conductor at potential V_0 , between two planes, B, B' , at zero potential, and at distance $2D$ asunder, on each of which the electric density is $\frac{1}{2}\rho$. For brevity, I shall denote the insulated conductor by I .

Its electrostatic capacity per unit area of its medial plane (the plane of the original infinitely thin bars) is ρ/V_0 .

7. This conductor, I , is symmetrical on each side of its medial plane, and consists either of an infinite number of isolated parallel bars, each surrounding one of the original infinitely thin bars, or of a plate symmetrically corrugated on its two sides, with maximum and minimum thicknesses respectively at the places of the infinitely thin bars, and the lines midway between them. For the case of isolated bars, let $2c$ be the diameter of each, in the medial plane. Then, to find V_0 , we must put $x = \pm c$ and $z = 0$, in (4). Thus we find

$$V_0 = 2\pi\rho \left(\frac{a}{2\pi} \log \frac{1}{4 \sin^2 \frac{\pi c}{a}} + D \right) \dots \dots \dots (5).$$

Hence the electrostatic capacity of I in the circumstances is

$$1/\left\{ 2\pi \left(D + \frac{a}{2\pi} \log \frac{1}{4 \sin^2 \frac{\pi c}{a}} \right) \right\} \dots \dots \dots (6),$$

which is greater or less than $1/(2\pi D)$, the electrostatic capacity that it would have if reduced to its medial plane, according as $c >$ or $< \frac{1}{2}a$.

The conductor I , to be a grating, implies $c < \frac{1}{2}a$, or $\sin^2 \frac{\pi c}{a} < 1$, and therefore requires that

$$V_0 > 2\pi\rho \left(D - \frac{a}{2\pi} \log 4 \right) = 2\pi\rho (D - .22a) \dots \dots \dots (7).$$

When V_0 exceeds this critical value, the conductor I is the continuous plate corrugated on each side, which was described in § 7. The critical value corresponds to an intermediate case of a plate so deeply furrowed on each side as to be just cut through by its two surfaces crossing at right angles; and (7) shows that the electrostatic

capacity of the conductor I so constituted is equal to that of a plane sheet of thickness

$$2a \log [2^2]/(2\pi), \text{ or } \cdot 44a \dots\dots\dots (8),$$

insulated midway between the two earth plates B, B', at the same distance asunder as they had with I between them.

8. By (4), (5), and (7), we have for the equation of the surface constituting the two sides of I in this critical case,

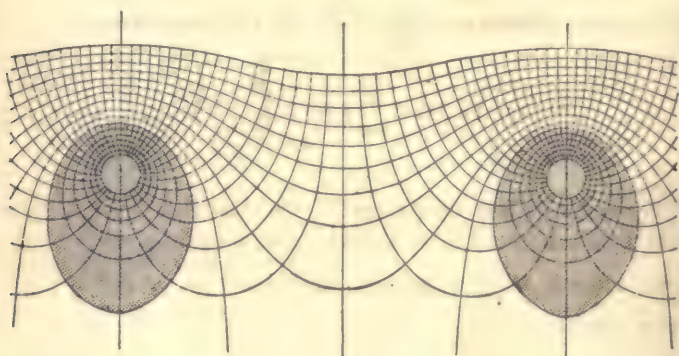
$$e^{mx} - 2 \cos mx + e^{-mx} = 4 \dots\dots\dots (9).$$

Taking double the positive value of x which this gives when $x = 0$, we find

$$2a \log [(1 + \sqrt{2})^2]/(2\pi), \text{ or } \cdot 562a \dots\dots\dots (10)$$

as the maximum thickness of I. This is $\log (1 + \sqrt{2})/\log 2$, or 1.273, times the amount shown in (8) for the thickness of the plane-sided plate of equal electrostatic capacity; which is just such a relation as is expected before calculation!

9. If $\phi(z, x)$ denote what V becomes when in place of mD we substitute $-mx$ in (4), we have the potential due to a uniform electrical force ρam , or $2\pi\rho$, added to the z -component, of the force due to the grating with its given charge of ρa quantity per unit length of each bar; and it is the equipotentials and lines of force of this system that are represented in Maxwell's diagram of Plate XIII, reproduced here.



In it the resultant force for infinitely large positive values of z is parallel to OZ , and of constant value $4\pi\rho$; and it is zero for infinitely large negative values of z . The approximation to these values is very close, at only so moderate a distance as a on either side of the grating.

10. Choosing, in the system of §6, any one of the multiple-oval

equipotentials around the infinitely thin bars, that indicated by the shading, for example which I have added to Maxwell's diagram, let c be the distance from the infinitely thin primary bar within it, at which it is cut by the plane of the primary bars. By putting, in the expression for $\phi(z, x)$, $z = 0$, and $x = c$, we find

$$\phi(0, c) = \rho a \log \frac{1}{4 \sin^2 \frac{\pi c}{a}} \dots \dots \dots (11)$$

as the potential at the surface of each of these chosen ovals. Construct now each of these ovals in metal, and let the supposed uniform force, $2\pi\rho$, be produced by uniform electrification of density $-\rho$, on a metal plane, B, at any great distance, b , on the positive side of the grating. We thus construct a grating of thick bars of oval-shaped cross section which, when electrified with the same quantity of electricity as that which we gave initially to the infinitely thin bars, and subjected to the influence of the equal quantity of negative electricity on B, has $\phi(z, x)$ for potential through external space from B ($z = b$), to infinite distance on the other side of the grating ($z = -\infty$), and has for potential through all the portions of space within the surfaces of the grate-bars the constant value expressed by (11). In this system the potential, for positive values of z great in comparison with a , is, by (4) with $-mz$ instead of $+mD$,

$$\phi(z, x) = -4\pi\rho z \dots \dots \dots (12).$$

The difference of potentials between B and the grating is, by (6) and (5),

$$\phi(0, c) - \phi(b, x) = 4\pi\rho \left(b + \frac{a}{4\pi} \log \frac{1}{4 \sin^2 \frac{\pi c}{a}} \right) \dots \dots (13).$$

Hence the electrostatic capacity of the mutually insulated system, B, and the grating of oval-shaped bars is equal to the capacity of a pair of parallel planes, B and a plane at a distance beyond the plane of the primitive infinitely thin bars equal to

$$\frac{a}{4\pi} \log \frac{1}{4 \sin^2 \frac{\pi c}{a}} \dots \dots \dots (14).$$

11. If in (4) we put $-nz$ in place of $+mD$, we have the potential of a system in which besides the electricity of the primary bars there is distant electricity such as all in all to give at great enough distances on the two sides of the primitive bars uniform fields of z -force respectively equal to

$$\rho(m+n), \text{ for } z = +\infty; \text{ and } \rho(m-n), \text{ for } z = -\infty, \dots (15).$$

If, in (4) with $-nz$ instead of $+mD$, we put

$$e^{-V/\rho a} = C \dots\dots\dots (16)$$

we find, as the equation of the equipotential surfaces,

$$-2 \cos mx + e^{ms} + e^{-ms} = C e^{-ns} \dots\dots\dots (17).$$

By taking $n = 0$, or $n = m$, we fall back on the cases of §§ 5-8, and §§ 9, 10, respectively.

12. To find an approximate equation for the equipotentials at distances around primitive bars small in comparison with a , the distance from bar to bar, let x and z be so small that we may neglect all powers of mx , mz , and nz , above the square, which implies that C is small of the same order as $(mx)^2$ and $(mz)^2$, (11) becomes

$$\left\{ \begin{array}{l} x^2 + \left(\frac{z + \frac{1}{2}nr^2}{1 + \frac{1}{4}n^2r^2} \right)^2 = r^2 \left(1 + \frac{1}{4}n^2r^2 \right) \\ \text{where } r^2 \text{ denotes } m^{-2}C. \end{array} \right\} \dots\dots\dots (18).$$

This shows that, to the degree of approximation in which we neglect cubes and higher powers of mx , mz , nz , the equipotential is a row of elliptic cylinders of eccentricity $nr/\sqrt{2}$, with their greater diametral planes perpendicular to the plane of the primitive bars. When $n = 0$, the equipotential is a row of circular cylinders having the primitive bars for their axes; and this is true to the higher approximation in which we need only neglect powers above the cubes of mx and mz , as we see by going back to equation (17), with $n = 0$.

13. The conclusions of § 12 are useful for detailed investigation of the screening effect of plane gratings of circular or elliptic, straight parallel bars electrified with given quantities of electricity and placed with their planes perpendicular to the lines of force in a uniform field of force, and to corresponding problems in which potentials are given, as in Maxwell's §§ 203-205.

14. Instead of a single row of parallel equidistant infinitely thin bars in one plane, let us take for primitives two or more such rows, parallel or not parallel, all in one plane or not in one plane. We may thus form an endless variety of force-systems available for illustrating or helping to solve problems which may occur. Towards the several problems of electric screening we find important contributions by considering in two parallel planes rows of primitive lines parallel to one another for one case and perpendicular to one another for another case. The consideration of three rows of primitive lines in one plane, dividing it into equal and similar triangles alternately oriented in

opposite directions, leads to a complete theory of electrostatic screening by a triangular lattice of metallic wire or ribbon. The fundamental potential formula for this system obtained by summation of expressions, each given by an application of (4) to one of the three rows, is

$$V = \log \frac{1}{(\epsilon^{lz} - 2 \cos lp + \epsilon^{-lz})^{\pi a} (\epsilon^{mz} - 2 \cos mq + \epsilon^{-mz})^{\rho b} (\epsilon^{nz} - 2 \cos nr + \epsilon^{-nz})^{\sigma c}} + 2\pi(\pi + \rho + \sigma)D \dots (19);$$

where a, b, c denote the intervals between the successive lines of the three systems; $\pi a, \rho b, \sigma c$, the quantities of electricity per unit length of bar in the three systems; p, q, r, z , special coordinates of the point for which (19) expresses the potential, viz., z , its distance from the plane of the primitive bars, and p, q, r its distances from three planes drawn perpendicular to this plane through a bar of each of the three systems; D the value of $\pm z$ for planes on the two sides of the net for which the potential is zero; and, lastly,

$$l = 2\pi/a, m = 2\pi/b, n = 2\pi/c \dots \dots \dots (20).$$

For the present, however, we may confine ourselves to the case of two rows of primitive lines dividing a plane into squares and charged, both rows, with equal quantities, $\frac{1}{2}\rho a$, of electricity per unit length. The potential formula, a particular case of (19), is

$$V = \frac{1}{2}\rho a \left[\log \frac{1}{(\epsilon^{mx} - 2 \cos mx + \epsilon^{-mx})(\epsilon^{my} - 2 \cos my + \epsilon^{-my})} + 2mD \right] (21).$$

15. The consideration of the equipotentials of this surface is very interesting. The equipotential lines in the plane of the primitive bars are given by the equation

$$16 \left(\sin \frac{\pi x}{a} \sin \frac{\pi y}{a} \right) \frac{\pi y^2}{a} = \epsilon^{-2V/\rho a} \dots \dots \dots (22).$$

16. Considerations quite analogous to those of §§ 6, 7, 8, and again the other considerations analogous to those of §§ 9—13, are, after the full explanations there given, easily completed so as to formulate a full theory of electrostatic capacity and electrostatic screening for square nets of wire exposed to electric action giving uniform fields of force at distances on each or on one side of the plane of the net considerable in comparison with a , the side of each square.

17. In what follows we shall for brevity call any thin sheet, whether plane or not plane, which answers to the description contained in the title of this paper, a *perforated sheet* or a *perforated surface*; understanding that its radii of curvature are everywhere large in com-

parison with its thickness. The diameters of holes must be large in comparison with the thickness in order that the approximations which we use below may be valid. We shall call the electric density of a perforated sheet the total quantity of electricity with which it is electrified, reckoned per unit area of continuous surface approximately agreeing with it, and passing through the middle of cage bars, bosses, &c. This continuous surface I shall call the medial surface, or sometimes, for brevity, *the medial*.

18. In what precedes we have virtually a complete investigation of the screening effect of a homogeneous plane perforated sheet against the electric force of a uniform field with lines of force perpendicular to the plane. Let it now be required to find the screening effect of a non-plane perforated sheet against a uniform field of electrostatic force, and of a perforated sheet S , plane or not plane, against the electrostatic force of any given electrified bodies.

19. Let ϕ be the potential of the given electrified bodies at any point (x, y, z) of the space occupied by S , and let ρ be the unknown electric density of S at (x, y, z) , under the influence of those bodies. To make the problem of finding ρ determinate, we might suppose either the total quantity of electricity on S , or the potential at which its metal is kept, to be given. We shall take the latter supposition, and call the given potential C .

20. Let ϕ denote the potential which would be produced by the electricity of S if it were spread continuously over the medial with electric density equal to ρ at (x, y, z) ; and let

$$\phi + \mu\rho \dots\dots\dots (23)$$

denote the potential in the metal of S , due to the actual distribution of electricity on its surface.

21. To understand the meaning of this notation (μ), consider a large area around (x, y, z) , so large that its border is very distant from (x, y, z) in comparison with the thickness of the sheet, and with the diameters of its apertures, but not so large as to deviate sensibly from the tangent plane at (x, y, z) . Let the electricity of all the surface of S beyond A be changed from the imagined continuous distribution to the actual distribution on the surface of the perforated metal. This change will make no sensible difference in the potential at (x, y, z) . Next, let the imagined continuous distribution of uniform electric density ρ , over the continuous area A , be changed to the actual distribution of the same quantity over the surface of the perforated metal of the porous sheet A . The augmentation of potential at (x, y, z) produced by this charge is what we denote by $\mu\rho$, where μ is a coefficient depending on the shapes and magnitudes of the perforations, that is to say, on the complex surface of the perforated metal. It would be zero if there were no perforations, and we shall see that the

greater it is the less is the screening efficiency. We shall therefore call μ the electric permeability, and μ^{-1} the electric screening efficiency of the perforated sheet. The sheet is homogeneous as to permeability or screening efficiency if μ has the same value for all parts of it, but we need not assume this to be the case; on the contrary, we shall suppose μ to be any known function of (x, y, z) . In §§ 5—16 we have the explanations necessary for determining μ in the various cases of gratings and nets there described. For similarly perforated surfaces, the values of μ are as the linear dimensions of a perforation or of the bars or bosses of the structures.

22. The equation of electric equilibrium is

$$\phi + \mu\rho = K \text{ (a constant) } \dots\dots\dots (24),$$

when S, being insulated and electrified, is not under the influence of any other electrified matter.

It is

$$\phi + \mu\rho = K - V \dots\dots\dots (25),$$

when S is under the influence of any given electrified bodies producing a given potential, V , at (x, y, z) .

23. As a first example, going back to (24), let μ be such that ϕ shall be constant. This makes, if we denote by k a constant,

$$\mu = k\phi/\rho \dots\dots\dots (26),$$

(k being a constant), which means that the screening efficiency is, in different places of S, inversely proportional to the electric density at similarly situated places of a continuous electrified conductor of the same shape as S. Let, for instance, S be an ellipsoid; then, if the sizes of the perforations be inversely proportional to the perpendicular from the centre to the tangent plane, (26) is satisfied. Generally, to fulfil this condition, the net must be finer in the more convex and more projecting parts, and coarser in the flatter and less projecting parts.

24. If any perforated conductor or cage, S, fulfilling the condition of § 23, be electrified and insulated away from the disturbing influence of other conductors, or electrified bodies, the charge distributes itself so as to have in every part the same quantity per unit area of the medial, as a smooth continuous metallic surface agreeing with the medial and electrified with the same total quantity. When the medial is a closed surface, the electricity on the perforated surface does not confine itself to the parts of it outside the medial: on the contrary, when the apertures are very wide in comparison with diameters of cage-bars, bosses, &c., the electricity distributes itself almost equally on the parts of the complex surface inside and outside the medial.

25. Seeing that the electric density (as defined in § 17) is the

same for a perforated surface fulfilling the condition of § 23 as for the medial constructed in continuous metal, we naturally ask the question, what then is the difference between the two cases, if any, besides the fact of the electricity being equally but very unequally distributed over the outer and inner portions of the complex surface in one case, and equally over the outside of the smooth medial in the other? There is a very important and interesting difference. The electrostatic capacity of the perforated conductor, S , is less, in the ratio of 1 to $1 + k$, than that of the medial constructed in continuous metal; as we see by (23) and (26).

26. As a sub-example, suppose S to be a spherical surface. If homogeneously perforated, it will fulfil the condition of § 23: and if its screening efficiency is the same as that of a grating of parallel bars (circular cross section of diameter $2r$; distance from centre to centre a), we have, by (5) of § 7, when $\pi c/a$ is very small,

$$\mu = 2a \log \frac{a}{2\pi c} \dots\dots\dots (27).$$

Now, S being spherical, if R denotes its radius, we have (§ 20)

$$\phi = 4\pi R\rho \dots\dots\dots (28).$$

Hence, by (26) and (27),

$$k = \frac{a}{2\pi R} \log \frac{a}{\pi c} = \frac{1}{N} \log \frac{a}{2\pi c} \dots\dots\dots (29),$$

where N denotes the number of bars in the equatorial belt of the cage of § 27 below.

27. To illustrate a realisation of § 26, let a spherical cage be made up of a narrow equatorial belt of approximately straight parallel bars of diameter $2c$, and distance from middle of one bar to middle of next, a ; completed by polar caps (nearly hemispheres) of thin metal perforated so as to have everywhere the same effective electric screening efficiency $1\{2a \log (a/2\pi c)\}$.

Suppose, for instance, the bars to be of "No. 18 gauge" ($2c = 0.122$ cm.) and $a = 5$ cm. We have

$$\log (a/2\pi c) = \log 13 = 2.57.$$

Hence, for this case, and any other in which the ratio a/c is the same, we have, by (27) and (29),

$$\mu = 5.14 a \dots\dots\dots (30),$$

$$k = 0.409 \frac{a}{R} \dots\dots\dots (31).$$

Thus, if $a = 5$ cm., and $R = 50$ cm., $k = 0.0409 \doteq \frac{1}{24}$; and (§ 25) the electrostatic capacity of the spherical cage $\frac{2}{3}$ of that of a simply continuous spherical surface of the same magnitude.

28. Let now an electrified metal globe, or globe of insulating material uniformly electrified, G, be insulated concentrically within S. It may be of any magnitude, large or small, provided only that the interval between the two surfaces be at least two or three times the diameter of the largest of the perforations of S. Let S be connected with the earth, and let Q denote the quantity of (positive) electricity with which G is electrified, and Q' the quantity of the opposite electricity which it induces on S. The potential in the metal of S due to Q' is, by (23),

$$-\left(\frac{Q'}{R} + \mu \frac{Q'}{4\pi R^2}\right) \dots\dots\dots (32).$$

This, added to Q/R, the potential due to G, must be zero, and therefore

$$Q = Q \left(1 + \frac{\mu}{4\pi R}\right) \dots\dots\dots (33),$$

or, by (26), $Q = Q'(1 + k) \dots\dots\dots (34).$

Hence, in the particular case of § 27 (31),

$$Q = Q' \left(1 + 0.0409 \frac{a}{R}\right) \dots\dots\dots (35);$$

and when $R = 10a$, we find $Q - Q' \doteq \frac{1}{25} Q$, and conclude that the effect of S, earthed, with G electrified and insulated within it, is just 4 per cent. of the effect of G unscreened.

29. If S is connected with the earth, and supported at a height above the earth equal to at least six or eight times its diameter, the quantity of electricity (positive in fine weather) induced on it will be $1/(1 + k)$ of that which would be induced on a simply continuous metal globe of the same size. Hence the potential at any point of the air within S at not less distance inwards than $2a$ will be $k/(1 + k)$ of the undisturbed atmospheric potential at the same height above the ground, or 5 per cent. in our particular case. This is quite in accordance with the imperfectness of the screening effect against atmospheric electricity found by Roiti* within earthed wire cages, supported at a considerable height above the ground, by a bracket attached to the top of a wall of a building in Florence, tested by a water-dropper with its nozzle inside the cage con-

* "Osservazioni Continue della Elettricit  Atmosferica" ('Pubblicazioni del R. Istituto di Studi Superiori in Firenze'), Florence, 1884.

nected by an insulated wire with a quadrant electrometer in the buildings.

30. The problem of finding the distribution of electricity on a spherical cage, of equal electric permeability, μ , in all parts of its surface, formulated in (25) of § 22, is easily solved by aid of spherical harmonics. Confining ourselves for brevity to the case of external influencing bodies, let their potential at any point, P, within S be

$$V = -\Sigma S_i \frac{r^i}{R^i} \dots\dots\dots (36),$$

where S_i denotes a given spherical surface-harmonic of order i , and r the distance of P from the centre of S. And ρ_i , denoting an unknown surface-harmonic of order i , let

$$\rho = \Sigma \rho_i \dots\dots\dots (37)$$

be the harmonic expression for ρ , the required electric density. Going back to § 20 for the definition of ϕ , we find, by the elements of spherical harmonics,

$$\phi = \Sigma \frac{4\pi\rho_i}{2i+1} \frac{r^i}{R^{i-1}} \dots\dots\dots (38).$$

Hence, by (25),

$$\rho_0 = \frac{K+S_0}{4\pi R+\mu} \dots\dots\dots (39),$$

$$\rho_i = \frac{(2i+1)S_i}{4\pi R} \frac{1}{1+\frac{(2i+1)\mu}{4\pi R}} \dots\dots\dots (40),$$

and

$$\phi = K + \Sigma \frac{1}{1+\frac{(2i+1)\mu}{4\pi R}} \frac{S_i r^i}{R^i} \dots\dots\dots (41).$$

In (39) we have virtually the same result as in (33). The approximation on which we are founding in §§ 17—29 is valid in (40) and (41) only for values of i small in comparison $2\pi R/a$: but, as in virtue of greatness of the logarithm for the case formulated in (27), μ may be great in comparison with a ; and therefore the denominator of (40) need not be only infinitesimally greater than unity, and may be any numeric however great.

31. Taking $S_1 r = x$, $S_2 = 0$, $S_3 = 0 \dots\dots$, we see by (41) that if an insulated unelectrified spherical cage be brought into a uniform field of electric force, X (that of atmospheric electricity, for example, at any height above the ground exceeding five or six diameters of the cage), the force within the cage is

$$X - \frac{X}{1 + \frac{3\mu}{4\pi R}} \dots\dots\dots (42),$$

or, according to (27), and (29),

$$X - \frac{X}{1 + \frac{3a}{2\pi R} \log \frac{a}{2\pi c}}, \text{ or } X - \frac{X}{1 + \frac{3}{N} \log \frac{a}{2\pi c}} \dots\dots (43).$$

This result is also applicable to a hemispherical screen of radius R , simply placed on the ground. For the particular proportions of § 27, it makes the force under the hemispherical cage $\frac{1}{5}$ of the undisturbed force outside. A cage of ordinary gardener's (anti-rabbit) hexagonal wire-net (of $5\frac{1}{2}$ cm. from parallel to parallel) cannot be very different from this. If, instead of the radius being 50 cm. it be 200, but the cage still of the same net, the force inside would be only 3 per cent. of the undisturbed force outside.

32. In every case the force at any distance from the perforated surface, on either side of it, more than the diameter of a perforation, is, as is easily proved by Fourier's methods, very nearly the same as if the electricity were spread equably over the medial surface, with the same quantity per unit area of the medial as the grating has in each part of it. Hence, in the case of § 31, the force is uniform throughout the interior of the cage, except within distances from the net of two or three times the aperture. Hence a second screen, similar but slightly smaller, placed inside the first will reduce the force farther in the same ratio; so that, if eX denote the force inside the single screen, the force inside the inner screen when there are two will be e^2X , provided the distance between the two is nowhere less than the diameter of the perforation. Thus, with screens such as those in the last particular case of § 31, the force inside the inner screen would be only 9/10,000 of the undisturbed force far enough outside the outer. The two screens, if placed close together, so as to narrow the apertures as much as possible, would have little more than double the screening efficiency of either singly, as we may judge from (27) of § 26, and from (21) of § 14. The principle that, to duplicate a screen with best advantage, the two screens should be placed, not in one surface but in two, with not less distance between them than the diameter of their apertures, is not only theoretically interesting, but is of great practical importance in the screening of electrometers against disturbing electric force.

33. Questions analogous to those of §§ 26—32, but for circular cylindric (mouse-mill) cages of equidistant parallel bars, instead of the spherical or hemispherical cage which we have been considering, are readily answered by the simpler work corresponding to that of

§ 30 (with $\sin i\theta$ and $\cos i\theta$ instead of spherical harmonics). But it deserves more complete synthetic investigation, not limited by the approximations of §§ 21, 22, if for no other reason, because of Hertz's mouse-mill. This must, however, be reserved for a future communication. Meantime, it is worth saying that sudden variations of electric current, or alternating electric currents, distribute themselves between different straight parallel conductors in the same proportion as static electrification is distributed in corresponding electrostatic arrangements, whenever the suddenness, or the frequency, is sufficient to cause the impedance by mutual induction of the separate parallel conductors (and therefore, *a fortiori*, the impedance by self-induction of each) to be very large in comparison with ohmic resistance. Hence Hertz's mouse-mill screening follows (though by utterly different physical action), simply the electrostatic law, except in any case in which his wave-length is less than a considerable multiple of the diameter of his mouse-mill.

II. "On Variational Electric and Magnetic Screening." By Sir W. THOMSON, P.R.S. Received April 1, 1891.

1. A screen of imperfectly conducting material is as thorough in its action, when time enough is allowed it, as is a similar screen of metal. But if it be tried against rapidly varying electrostatic force, its action lags. On account of this lagging, it is easily seen that the screening effect against periodic variations of electrostatic force will be less and less, the greater the frequency of the variation. This is readily illustrated by means of various forms of idiostatic electrometers. Thus, for example, a piece of paper supported on metal in metallic communication with the movable disc of an attracted disc electrometer annuls the attraction (or renders it quite insensible) a few seconds of time after a difference of potential is established and kept constant between the attracted disc and the opposed metal plate, if the paper and the air surrounding it are in the ordinary hygrometric condition of our climates. But if the instrument is applied to measure a rapidly alternating difference of potential, with equal differences on the two sides of zero, it gives very little less than the same average force as that found when the paper is removed and all other circumstances kept the same. Probably, with ordinary clean white paper in ordinary hygrometric conditions, a frequency of alternation of from 50 to 100 per second will more than suffice to render the screening influence of the paper insensible. And a much less frequency will suffice if the atmosphere surrounding the paper is artificially dried. Up to a frequency of millions per second, we may safely say that, the greater the frequency, the more perfect is the annulment of

screening by the paper; and this statement holds also if the paper be thoroughly blackened on both sides with ink, although possibly in this condition a greater frequency than 50 to 100 per second might be required for practical annulment of the screening.

2. Now, suppose, instead of attractive force between two bodies separated by the screen, as our test of electrification, that we have as test a faint spark, after the manner of Hertz. Let two well insulated metal balls, A, B, be placed very nearly in contact, and two much larger balls, E, F, placed beside them, with the shortest distance between E, F sufficient to prevent sparking, and with the lines joining the centres of the two pairs parallel. Let a rapidly alternating difference of potential be produced between E and F, varying, not abruptly, but according, we may suppose, to the simple harmonic law. Two sparks in every period will be observed between A and B. The interposition of a large paper screen between E, F, on one side, and A, B, on the other, in ordinary hygrometric conditions, will absolutely stop these sparks, if the frequency be less than, perhaps, 4 or 5 per second. With a frequency of 50 or more, a clean white paper screen will make no perceptible difference. If the paper be thoroughly blackened with ink on both sides, a frequency of something more than 50 per second may be necessary; but some moderate frequency of a few hundreds per second will, no doubt, suffice to practically annul the effect of the interposition of the screen. With frequencies up to 1000 million per second, as in some of Hertz's experiments, screens such as our blackened paper are still perfectly transparent, but if we raise the frequency to 500 million million, the influence to be transmitted is light, and the blackened paper becomes an almost perfect screen.

3. Screening against a varying magnetic force follows an opposite law to screening against varying electrostatic force. For the present, I pass over the case of iron and other bodies possessing magnetic susceptibility, and consider only materials devoid of magnetic susceptibility, but possessing more or less of electric conductivity. However perfect the electric conductivity of the screen may be, it has no screening efficiency against a steady magnetic force. But if the magnetic force varies, currents are induced in the material of the screen which tend to diminish the magnetic force in the air on the remote side from the varying magnet. For simplicity, we shall suppose the variations to follow the simple harmonic law. The greater the electric conductivity of the material, the greater is the screening effect for the same frequency of alternation; and, the greater the frequency, the greater is the screening effect for the same material. If the screen be of copper, of specific resistance 1640 sq. cm. per second (or electric diffusivity 130 sq. cm. per second), and with frequency 80 per second, what I have called the "mhoic effective

thickness" is $0\cdot71$ of a cm.; and the range of current intensity at depth $n \times 0\cdot71$ cm. from the surface of the screen next the exciting magnet is e^{-n} of its value at the surface.

Thus (as $e^3 = 20\cdot09$) the range of current-intensity at depth $2\cdot13$ cm. is $\frac{1}{20}$ of its surface value. Hence we may expect that a sufficiently large plate of copper of $2\frac{1}{4}$ cm. thick will be a little less than perfect in its screening action against an alternating magnetic force of frequency 80 per second.

4. Lord Rayleigh, in his "Acoustical Observations" ('Phil. Mag.,' 1882, first half-year), after referring to Maxwell's statement, that a perfectly conducting sheet acts as a barrier to magnetic force ('Electricity and Magnetism,' § 665), describes an experiment in which the interposition of a large and stout plate of copper between two coils renders inaudible a sound which, without the copper screen, is heard by a telephone in circuit with one of the coils excited by electromagnetic induction from the other coil, in which an intermittent current, with sudden, sharp variations of strength, is produced by a "microphone clock" and a voltaic battery. Larmor, in his paper on "Electromagnetic Induction in Conducting Sheets and Solid Bodies" ('Phil. Mag.,' 1884, first half-year), makes the following very interesting statement:—"If we have a sheet of conducting matter in the neighbourhood of a magnetic system, the effect of a disturbance of that system will be to induce currents in the sheet of such kind as will tend to prevent any change in the conformation of the tubes [lines] of force cutting through the sheet. This follows from Lenz's law, which itself has been shown by Helmholtz and Thomson to be a direct consequence of the conservation of energy. But if the arrangement of the tubes [lines of force] in the conductor is unaltered, the field on the other side of the conductor into which they pass (supposed isolated from the outside spaces by the conductor) will be unaltered. Hence, if the disturbance is of an alternating character, with a period small enough to make it go through a cycle of changes before the currents decay sensibly, we shall have the conductor acting as a screen.

"Further, we shall also find, on the same principle, that a rapidly rotating conducting sheet screens the space inside it from all magnetic action which is not symmetrical round the axis of rotation."

Mr. Willoughby Smith's experiments on "Volta-electric induction," which he described in his inaugural address to the Society of Telegraph Engineers of November, 1883, afforded good illustrations of this kind of action with copper, zinc, tin, and lead, screens, and with different degrees of frequency of alternation. His results with iron are also very interesting: they showed, as might be expected, comparatively little augmentation of screening effect with augmentation

of frequency. This is just what is to be expected from the fact that a broad enough and long enough iron plate exercises a large magneto-static screening influence; which, with a thick enough plate, will be so nearly complete that comparatively little is left for augmentation of the screening influence by alternations of greater and greater frequency.

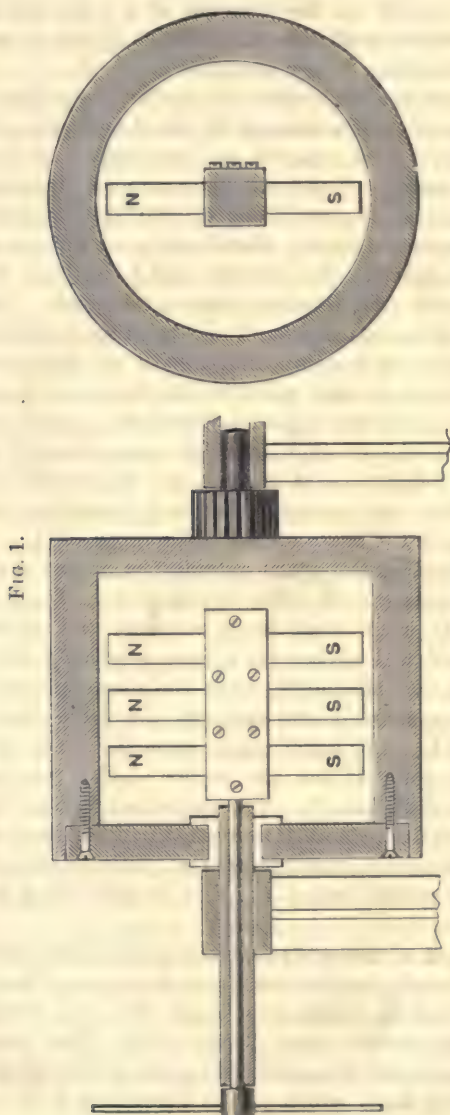
5. A copper shell closed around an alternating magnet produces a screening effect which on the principle of § 3 we may reckon to be little short of perfection if the thickness be $2\frac{1}{4}$ cm. or more, and the frequency of alternation 80 per second.

6. Suppose now the alternation of the magnetic force to be produced by the rotation of a magnet *M* about any axis. First, to find the effect of the rotation, imagine the magnet to be represented by ideal magnetic matter. Let (after the manner of Gauss in his treatment of the secular perturbations of the solar system) the ideal magnetic matter be uniformly distributed over the circles described by its different points. For brevity call *I* the ideal magnet symmetrical round the axis, which is thus constituted. The magnetic force throughout the space around the rotating magnet will be the same as that due to *I*, compounded with an alternating force of which the component at any point in the direction of any fixed line varies from zero in the two opposite directions in each period of the rotation. If the copper shell is thick enough, and the angular velocity of the rotation great enough, the alternating component is almost annulled for external space, and only the steady force due to *I* is allowed to act in the space outside the copper shell.

7. Consider now, in the space outside the copper shell, a point *P* rotating with the magnet *M*. It will experience a force simply equal to that due to *M* when there is no rotation, and, when *M* and *P* rotate together, *P* will experience a force gradually altering as the speed of rotation increases, until, when the speed becomes sufficiently great, it becomes sensibly the same as the force due to the symmetrical magnet *I*. Now superimpose upon the whole system of the magnet, and the point *P*, and the copper shell, a rotation equal and opposite to that of *M* and *P*. The statement just made with reference to the magnetic force at *P* remains unaltered, and we have now a fixed magnet *M* and a point *P* at rest, with reference to it, while the copper shell rotates round the axis around which we first supposed *M* to rotate.

8. A little piece of apparatus, constructed to illustrate the result experimentally, is submitted to the Royal Society and shown in action. In the copper shell is a cylindric drum, 1.25 cm. thick, closed at its two ends with circular discs 1 cm. thick. The magnet is supported on the inner end of a stiff wire passing through the centre of a perforated fixed shaft which passes through a hole in one end of

the drum, and serves as one of the bearings; the other bearing is a rotating pivot fixed to the outside of the other end of the drum. The accompanying sections, drawn to a scale of three-fourths full size, explain the arrangement sufficiently. A magnetic needle outside, deflected by the fixed magnet when the drum is at rest, shows a great diminution of the deflection when the drum is set to rotate.



If the (triple compound) magnet inside is reversed, by means of the central wire and cross bar outside, shown in the diagram, the magnetometer outside is greatly affected while the copper shell is at rest; but scarcely affected perceptibly while the copper shell is rotating rapidly.

9. When the copper shell is a figure of revolution, the magnetic force at any point of the space outside or inside is steady, whatever be the speed of rotation; but if the shell be not a figure of revolution, the steady force in the external space observable when the shell is at rest becomes the resultant of the force due to a fixed magnet intermediate between M and I compounded with an alternating force with amplitude of alternation increasing to a maximum, and ultimately diminishing to zero, as the angular velocity is increased without limit.

10. If M be symmetrical, with reference to its northern and southern polarity, on the two sides of a plane through the axis of rotation, I becomes a null magnet, the ideal magnetic matter in every circle of which it is constituted being annulled by equal quantities of positive and negative magnetic matter being laid on it. Thus, when the rotation is sufficiently rapid, the magnetic force is annulled throughout the space external to the shell. The transition from the steady force of M to the final annulment of force, when the copper shell is symmetrical round its axis of rotation, is, through a steadily diminishing force, without alternations. When the shell is not symmetrical round its axis of rotation, the transition to zero is accompanied with alternations as described in § 8.

11. When M is not symmetrical on the two sides of a plane through the axis of rotation, I is not null; and the condition approximated to through external space with increasing speed of rotation is the force due to I , which is an ideal magnet symmetrical round the axis of rotation.

12. A very interesting simple experimental illustration of screening against magnetic force may be shown by a rotating disc with a fixed magnet held close to it on one side. A bar magnet held with its magnetic axis bisected perpendicularly by a plane through the axis of rotation would, by sufficiently rapid rotation, have its magnetic force almost perfectly annulled at points in the air as near as may be to it, on the other side of the disc, if the diameter of the disc exceeds considerably the length of the magnet. The magnetic force in the air close to the disc, on the side next to the magnet, will be everywhere parallel to the surface of the disc.

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III. "The Measurement of the Power given by any Electric Current to any Circuit." By W. E. AYRTON, F.R.S., Professor of Applied Physics in the City and Guilds of London Institute, and W. E. SUMPNER, D.Sc. Received March 16, 1891.

I.

During the meeting of the Electrical Congress at Paris in 1881, one of us* devised a method of using an electrometer for measuring the power given to any circuit by any current. The accuracy of the method is wholly independent of the nature of the circuit, which may possess self-induction, mutual induction capacity, and an E.M.F., as well as of the nature of the current, which may be constant, intermittent, or alternating, according to any function of the time. This method is the only electrical one published up to the present date the accuracy of which is not based on assumptions, either as regards the nature of the current or as regards the entire absence of self- and mutual induction from a circuit some portion of which is necessarily of a solenoidal form, or as regards the nature of the circuit the power given to which we desire to measure.

In view then of the present wide use of alternating currents for industrial purposes, it might have been expected that this electrometer method of measuring the power given by any intermittent or alternating current to an inductive circuit would have been extensively employed. Unfortunately, however, as pointed out by one of us in conjunction with Professor Perry,† the use of this method is restricted by the fact that Sir W. Thomson's quadrant electrometers do not generally obey the mathematical law given for these instruments in text-books,‡ as it was supposed they did when this electrometer method of measuring power was first suggested. And hence the main result that has, up to the present time, followed from the publication of this method has been the stimulation of inventive minds to devise forms of electrometers in which the text-book law is strictly fulfilled.

In 1888, Mr. Blakesley published a very ingenious method for using three dynamometers to measure the power given by an alternating

* This method was simultaneously arrived at independently by Professor Fitzgerald.

† 'Journal of Soc. of Tel. Engs. and Elects.,' vol. 17, 1888.

‡ We may mention that an investigation on Quadrant Electrometers has been going on from time to time at the Central Institution for the last five years, and we had hoped to have communicated the complete report long before this to the Royal Society.

current to the primary coil of a transformer. His original proof, a geometrical one, was based on various hypotheses, amongst others, that the primary and secondary currents and the magnetic flux were sine functions of the time.

Recently, one of us, in conjunction with Mr. Taylor, has published* an analytical proof showing that Mr. Blakesley's three dynamometer method of measuring power gives equally true results, whatever functions the currents and magnetic flux be of the time. There still however, remains a serious objection to this method, viz., that it assumes the absence of magnetic leakage in the transformer, or in other words, that the number of lines of force embraced by one convolution of the primary coil at any moment is the same as the number of lines of force embraced by one convolution of the secondary. Further, the three dynamometer method cannot be used to measure the power given to a single circuit, as the coils of one of the dynamometers have necessarily to be put in different circuits.

The employment of an electromagnetic wattmeter for the measurement of electric power is well known, and investigators have considered the error that is introduced into wattmeter measurements made with alternating currents on account of the fine-wire circuit of the wattmeter possessing self-induction. This fine-wire circuit usually consists of a suspended coil in series with a so-called non-inductive stationary high resistance, and various devices have been adopted by different experimenters to make the effective self-induction of this fine-wire circuit nought. One of the simplest of these devices we venture to think is that proposed by one of us in conjunction with Mr. Mather, and which consists in winding the stationary so-called non-inductive resistance in such a way that the capacity of this doubly-wound coil practically neutralises the effect of the self-induction of the suspended coil.

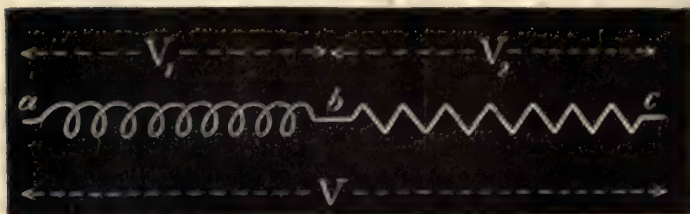
II.

Several months ago, however, while working at alternate current interference, we noticed that it was possible to employ an extremely simple method, based on the difference of phase of the P.D. and the current, for measuring the power by *any* current to *any* circuit. This method, which has since been in regular use in the laboratories of the Central Institution, is quite independent of any assumptions as to the nature of the current, or of the circuit, the power given to which it is desired to measure, and it has the further great advantage that the only measuring instrument required is the ordinary alternate-current voltmeter of commerce.

In series with the circuit *ab* (fig. 1), the power given to which we desire to measure, connect a non-inductive resistance *bc* of *r* ohms.

* Meeting of Physical Society, February 27, 1891.

FIG. 1.



Let V_1 , V_2 , and V be the readings of the voltmeter when applied between a and b , b and c , and a and c respectively; then, if W be the mean watts supplied to the circuit ab , we have in all cases, whatever the nature of the current, or of the circuit ab —

$$W = \frac{1}{2r} (V^2 - V_1^2 - V_2^2) \dots \dots \dots (1).$$

For, let v_1 , v_2 , and v be the instantaneous values of the P.D. between a and b , b and c , and a and c at some moment t ,

then $v = v_1 + v_2 \dots \dots \dots (2).$

If α be the current in amperes flowing through the circuit at time t , then αv_1 equals the watts w given to ab at that time. But

$$\alpha = \frac{v_2}{r},$$

since the resistance bc is non-inductive;

$$\therefore w = \frac{v_1 v_2}{r}.$$

Then, squaring (2) we have—

$$v^2 = v_1^2 + 2v_1 v_2 + v_2^2$$

$$\therefore w = \frac{1}{2r} (v^2 - v_1^2 - v_2^2).$$

Consequently
$$\int_0^T w dt = \frac{1}{2r} \left(\int_0^T v^2 dt - \int_0^T v_1^2 dt - \int_0^T v_2^2 dt \right),$$

or
$$W = \frac{1}{2r} (V^2 - V_1^2 - V_2^2),$$

the equation given above.

If the resistance of bc be not known, or if there be any fear that it may be changed by the passage of the current, then an ammeter (an

alternate current ammeter, of course, if alternate currents be employed) can be inserted in the circuit. Let A be the reading of this ammeter, and which represents the square root of the mean square of the current, then, for r in (1) we may substitute V_2/A , or

$$W = \frac{A}{2V_2} (V^2 - V_1^2 - V_2^2) \dots\dots\dots (3).$$

When employing this last formula, the non-inductive resistance bc may be that offered by incandescent lamps, since there is no objection to the resistance varying with different mean strengths of the current employed.

This voltmeter method of measuring power was arrived at quite independently of the electrometer method referred to above, but an examination of the electrometer method shows that it is practically equivalent to simultaneous measurements of three P.Ds.

III.

The method which we have described for measuring the power given by *any* current to *any* circuit may be conveniently employed for measuring the power given to an alternating-current arc, or to an alternating-current arc-lamp. It is known that an alternating-current arc requires a greater current than a direct-current arc to produce the same light with similar carbons; for example, a 10-ampere direct-current lamp requires $12\frac{1}{2}$ amperes, or 25 per cent. larger current, when used with an alternating current. In a masterly paper on "The Theory of Alternating Currents," read before the Society of Telegraph Engineers, on November 13th, 1884, Dr. Hopkinson refers to a law given by Joubert, that the difference of potential between the carbons in an alternating arc is of approximately constant numerical value throughout the period, and that it reverses sign discontinuously at each reversal of the current. Using this law as his basis, he works out mathematically some very curious relationships between the variations of current and P.D. with time.

Three of our senior students, Messrs. Kolkhorst, Thornton, and Weekes, have been making a number of experiments on the power supplied to alternating-current arcs by using the method of measuring power described above. From these experiments it would appear that the quality of the carbon employed affects materially the difference in phase between the currents passing through the arc and the P.D. between the carbons. If the arc be quite steady and only give out the rhythmic hum that accompanies a good arc, such as can be obtained with cored carbons of proper quality, the arc appears to act practically as a simple resistance, and M. Joubert's law does not hold. But if the arc be maintained between uncored carbons of poor

quality, and be hissing, there is considerable difference in phase between the current and the P.D. between the terminals; further, the experiments show that current is very far from being a sine function of the time, although produced by a dynamo whose E.M.F. normally follows a harmonic law.

We do not propose, in this communication, to enter at length into these experiments on alternate-current arcs, but a few examples of the experimental results that have been obtained will be interesting as illustrating the ready applicability of this new method of measuring power to such investigations.

In addition to the difference of phase of P.D. and current that may be produced in the arc itself, there is the electromagnet to be considered by which the distance between the carbons is usually regulated in arc lamps. This electromagnet will introduce lag between the P.D. at the terminals of the lamp and the current passing through the electromagnet and the arc in series; and hence, even although the arc be perfectly steady, we find, even in the case of a Brush lamp especially intended for alternate currents, that the true power supplied to the electromagnet and arc is 20 per cent. less than the product of the readings of the ammeter and the voltmeter attached to the lamp terminals, and which gives the square root of the mean product of the squares of the current and P.D.

If, however, the arc be between common carbons and be hissing, the difference, we find, is much greater. With cored carbons this Brush lamp requires a P.D. of about 35 volts to be maintained between its terminals, but if these cored carbons be replaced by common carbons and the arc be hissing, the P.D. between the terminals of the lamp at once rises to 45 or even 50 volts, although the current passing through the lamp and the amount of light given out remain practically as before. And then we find that the true power supplied to the lamp may be only one-half of the square root of the mean product of the squares of the current and P.D., so that the readings of the ammeter and voltmeter alone make the apparent power twice as great as the true power.

For the purpose of easily estimating the ratio of the true to the apparent power supplied, formula (3) may be thus written,

$$W = AV_1 \left\{ 1 - \frac{(V_1 + V_2 - V)(V_1 + V_2 + V)}{2V_1V_2} \right\} \dots\dots\dots (4),$$

from which we see that the expression in the brackets represents the ratio of the true to the apparent power supplied to the lamp or other circuit *ab* (fig. 1). Hence the percentage error made in assuming that the power supplied to any circuit was the product of the ammeter and voltmeter readings would be in all cases, whatever the nature of the current or of the circuit,

$$100 \frac{(V_1 + V_2 - V)(V_1 + V_2 + V)}{2V_1V_2} \dots\dots\dots (5).$$

The following are samples of the results obtained with a hand-regulated lamp, there being no electromagnet at all in series with

FIG. 2.



the arc (fig. 2). The carbons were not cored and the arc was hissing. The frequency was maintained at 200 periods per second.

Table I.

Square root of mean square				Percentage error in estimating power formula (5).
Of P.D. in volts between			Of current in amperes.	
<i>a</i> and <i>b</i> . <i>V</i> ₁ .	<i>b</i> and <i>c</i> . <i>V</i> ₂ .	<i>a</i> and <i>c</i> . <i>V</i> .	<i>A</i> .	
55·0	60·0	108·0	12·3	24·0
45·4	75·4	107·3	11·8	45·8

For the purpose of obtaining an idea of ϕ , the angle of phase difference produced by the hissing arc, between the current and the

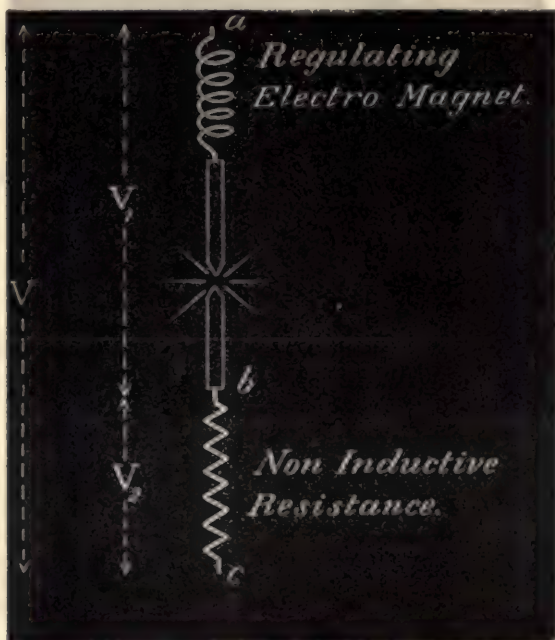
P.D., we may assume that the P.D. and current are sine functions of the time; then, as may be easily proved,

$$\cos \phi = \frac{V^2 - V_1^2 - V_2^2}{2V_1V_2} \dots\dots\dots (6),$$

and the values of ϕ for the two tests given above come out as $40^\circ 20'$ and $57^\circ 10'$. It will, of course, be observed that this assumption of a harmonic law for the P.D. and current for the purpose of obtaining some idea of the value of ϕ in no way affects the generality of the method for the measurement of the power, since this is based on no such assumption.

The following are samples of the results obtained with a Brush alternate-current lamp regulated by an electromagnet (fig. 3), the

FIG. 3.



carbons not being cored, and the arc hissing. The frequency was maintained at 200 periods per second.

Table II.

Square root of the mean square]				Percentage error in estimating power formula (5).	Lag between current and P.D.
Of P.D. in volts between			Of current in amperes.		
<i>a</i> and <i>b</i> . <i>V</i> ₁ .	<i>b</i> and <i>c</i> . <i>V</i> ₂ .	<i>a</i> and <i>c</i> . <i>V</i> .			φ.
64·8	58·0	108·4	13·0	44·0	56° 0'
59·8	64·2	107·4	12·0	50·5	60 20
55·0	67·3	107·4	10·6	47·0	58 30

The experiments already described tell us that a hissing arc may cause a considerable phase difference between the P.D. and the current, but they do not enable us to decide whether such an arc causes the current to lag behind the P.D., or to lead in front of it. To decide this point, that is, to decide whether a hissing arc acts like an inductive coil, or a condenser, a variety of experiments were made by putting induction or capacity in series with the arc. The following gives the result of one such experiment:—In series with a hand-regulated lamp (and, therefore, containing no electromagnet), was placed a condenser of 89 microfarads (fig. 4). Uncored carbons were used, and they were adjusted so that the arc was very short at first; the carbons were then not touched, and, as they burnt away, the arc grew longer and longer until it finally went out. The frequency was maintained at 200 periods per second.

FIG. 4.

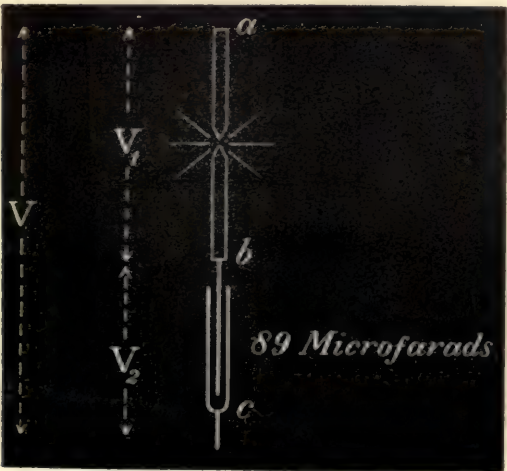


Table III.

Square root of mean square				Sum of $V_1 + V_2$.	Lag between current and P.D.	
E.M.F. of dynamo in volts.	Of P.D. in volts between				Of current in amperes.	
	a and b . V_1 .	b and c . V_2 .	a and c . V .		A .	
					ϕ .	
59	35.4	89.0	72.3	12.0	124.4	129°
	38.0	92.0	73.3	12.5	130.0	133
	51.2	104.5	74.3	14.0	155.7	135
	69.2	86.5	67.5	13.4	155.7	131

Comparing V with the E.M.F. of the dynamo, we see that the arc and the condenser together acted as a condenser on the whole; but, comparing V with $V_1 + V_2$, we see that the arc acted as an induction and not as a capacity.

It having been conclusively proved that a hissing arc with uncured carbons acts as an induction, it was interesting to compare the impedance it produces with the impedance produced by the ordinary regulating electromagnet of the lamp. The arc itself seen in fig. 3 was, therefore, short-circuited, and the following measurements made, V_1 now being the square root of the mean square of the P.D. between the terminals of the regulating electromagnet, V_2 as before that between the terminals of the non-inductive resistance, and V that between a and c , the arc, as already explained, being short-circuited. The frequency was maintained at 200 periods per second.

Table IV.

V_1 .	V_2 .	V .	A .
35.4	69.2	82.0	11.3
35.4	65.6	80.0	11.4

We have, then, P.D. measurements giving the phase difference of current and P.D. with the arc alone (Table I and fig. 2); with the arc and regulating electromagnet (Table II and fig. 3); and with the electromagnet alone (Table IV). Defining impedance in the usual way as the ratio of the square root of mean square of P.D. to the square root of mean square of current, we find from the two sets of results given on Table I, that

the impedance of the arc alone equals $\left\{ \begin{array}{l} 4.47; \\ 3.78; \end{array} \right.$

from the three sets of results given in Table II, that

the impedance of the arc and electromagnet equals $\left\{ \begin{array}{l} 4.87 \\ 5.15; \\ 5.14 \end{array} \right.$

and from the two sets of results given in Table IV, that

the impedance of the electromagnet alone equals. . $\left\{ \begin{array}{l} 3.14 \\ 3.16. \end{array} \right.$

In order to test whether the current follows a harmonic law, let us assume that it does, then find what result this assumption leads to, and, lastly, see whether the experiments confirm this result or not. Let, therefore, the instantaneous current at any moment be of the form

$$\frac{V_0}{\sqrt{(r^2 + L^2 p^2)}} \sin (pt - q),$$

where r is the effective resistance in each case, viz., the ratio of the true watts given to the circuit divided by the mean square of the current in amperes, and where p equals $2\pi n$, n being the number of periods per second. In each of the seven experiments referred to in Tables I, II, and IV, n was 200.

The seven values of r in ohms corresponding with the seven values of the impedance given above are for the

Arc alone $\left\{ \begin{array}{l} 3.42 \\ 2.07. \end{array} \right.$

Arc and electromagnet $\left\{ \begin{array}{l} 2.65 \\ 2.66. \\ 2.71 \end{array} \right.$

Electromagnet alone $\left\{ \begin{array}{l} 0.44 \\ 0.54. \end{array} \right.$

And, since the impedance equals $\sqrt{(r^2 + L^2 p^2)}$, if the harmonic law be true, the corresponding values of Lp are, for

Arc alone $\left\{ \begin{array}{l} 2.88 \\ 3.16. \end{array} \right.$

Arc and electromagnet $\left\{ \begin{array}{l} 4.08 \\ 4.41. \\ 4.37 \end{array} \right.$

Electromagnet alone $\left\{ \begin{array}{l} 3.12 \\ 3.11. \end{array} \right.$

But if the harmonic law hold for the current, the sum of $I\rho$ for the arc alone, plus the $I\rho$ for the electromagnet alone, must equal the $I\rho$ for the arc and electromagnet, since p has the same value in each case. Now it is obvious that the condition is far from being fulfilled with the numbers just given. Hence the current does not follow a harmonic law.

It is interesting to notice that the $I\rho$ for the hissing arc alone is actually greater than the $I\rho$ for the regulating electromagnet.

The values given above for r , being obtained by dividing the *true* watts by the mean square of the current in amperes, are the effective resistances in ohms—whether the current follows a harmonic law or not. Hence, by comparing the value of r for the regulating electromagnet alone with its resistance in ohms, measured with a steady current, we have a true measure of the waste of energy in the iron core of the electromagnet due to hysteresis and Foucault currents. Now the resistance of this electromagnet for a steady current is only 0.065 ohm; hence 90 per cent. of the energy given to the regulating electromagnet of this Brush lamp is wasted in heating its iron core when the frequency is 200. Here again we have a further illustration of the importance of being able to measure, by means of the simple method we have described, the power given by any current to any circuit.

Added March 31, 1891.

IV.

The Best Value to give to the Non-Inductive Resistance.

In cases where great accuracy is required in the measurement of the power given to a circuit, it is important to consider what value should be given to the non-inductive resistance (fig. 1), in order to reduce to a minimum any error that may arise from possible inaccuracies made in the three readings of the voltmeter, or on the graduation of its scale.

$$\text{Since} \quad W = \frac{1}{2r}(V^2 - V_1^2 - V_2^2),$$

$$dW = \frac{1}{r}(VdV - V_1dV_1 - V_2dV_2),$$

where dV , dV_1 , dV_2 are the errors made in the estimation of the three P.Ds.

Let

$$dV = \pm eV$$

$$dV_1 = \pm eV_1$$

$$dV_2 = \pm eV_2$$

where e is a small fraction, i.e., let the errors be each the same small fraction of the correct value, then the probable value of $(dW)^2$ is

$$\frac{1}{e^2} (V^2 e^2 V^2 + V_1^2 e^2 V_1^2 + V_2^2 e^2 V_2^2),$$

so that
$$\left(\frac{dW}{W}\right)^2 = 4e^2 \frac{V^4 + V_1^4 + V_2^4}{(V^2 - V_1^2 - V_2^2)^2} \dots\dots\dots (7).$$

Let the non-inductive resistance have such a value that

$$V_2 = xV_1 \dots\dots\dots (8),$$

V_2 being already defined, the square root of the mean square of the P.D. between its terminals, and V_1 the square root of the mean square of the P.D. between the terminals of the circuit the power given to which we desire to measure. Then we wish to find the value of x that will make dW/W a minimum.

Let ϕ be the angle of lag between the current in the circuit ac and the P.D. at the terminals of ab (fig. 1), then ϕ is the angle of lag between the P.D. at the terminals of ab and the P.D. at the terminals of bc . Hence, since

$$v = v_1 + v_2,$$

v , v_1 , and v_2 being the instantaneous values of the P.Ds.,

$$V^2 = V_1^2 + V_2^2 + 2V_1V_2 \cos \phi \dots\dots\dots (9).$$

Eliminating V , V_1 , and V_2 by means of equations (7), (8), and (9), we have

$$\left(\frac{dW}{W}\right)^2 = 4e^2 \frac{(1+x^2+2x \cos \phi)^2 + 1+x^4}{4x^2 \cos^2 \phi}.$$

Now $\cos \phi$ depends on the circuit, the power given to which we desire to measure, and is independent of x . Hence differentiating with respect to x and equating to nought in the usual manner, we find that x equal to unity makes $\frac{dW}{W}$ a minimum.

Hence, inaccuracies in the three readings of the voltmeter, or in the graduation of its scale, produce the least effect in this method of measuring power when the P.D. between the terminals of the non-inductive resistance is equal to the P.D. at the terminals of the circuit under test.

The next point to consider is, what is the percentage error made in measuring the power by this method compared with the percentage error made in reading one of the P.Ds.

Let x equal unity, then

$$\frac{dW}{W} = 2e \frac{\sqrt{2+4(1+\cos\phi)^2}}{2\cos\phi},$$

or

$$\frac{dW}{We} = \frac{\sqrt{2+4(1+\cos\phi)^2}}{\cos\phi}.$$

Now dW/We is the ratio of the percentage error made in measuring the power to the percentage error made in measuring one of the P.Ds. and the right-hand side of the last equation we find equals from 4 to 5 for the values of the lag angle ϕ that occur in ordinary practice. If then there were a positive or a negative error of 1 per cent. in each of the measurements of V , V_1 , and V_2 , there would be a probable error of from 4 to 5 per cent. in the measurement of the power. The probable percentage error in the measurement of the power being from 4 to 5 times the error in the measurement of each of the P.Ds. arises partly from the fact that the expression for W , being

$$\frac{1}{2r} (V^2 - V_1^2 - V_2^2),$$

depends directly on the difference in the mean squares of the P.Ds., and not on the difference of the square roots of the mean squares. And as all instruments that are graduated for measuring the square root of the mean square of an alternating P.D. such as a hot-wire voltmeter, an electrostatic voltmeter, &c., really measure the mean square and not the square root of the mean square directly, it would be better, if such an instrument were to be employed for the method of measuring power described in this paper, that it should be graduated in mean squares of P.Ds. and not in the square roots of the mean squares. In that case a similar line of reasoning to that employed above shows that the probable percentage error in the measurement of power by the method would be from 2 to 2.5 times the error in the measurement of each of the P.Ds.

It is, of course, clear that these errors to which we have been referring are not errors in any way essential to the method proposed for measuring power, since by the employment of an accurately graduated voltmeter, by exercising care in taking the readings, and if necessary, by repeating the measurements two or three times and taking the means of the observations, the power can be measured to any degree of accuracy desired.

V.

Approximate Calculation of the Power from the Three Readings of the Voltmeter.

The calculation of the power from formula (1) is easy, especially when the voltmeter is graduated to read the mean squares of the P.D.s. and not the square roots of the mean squares. If, however, as is usually the case, the scale is graduated in square roots, then even the trouble of taking the squares may be saved, when $V_1 + V$ does not differ much from V , by using the following method:—

Let the inductive resistance be arranged so that V_1 is nearly equal to V_2 , and let

$$V_1 + V_2 - V = yV_1;$$

then, since
$$\cos \phi = \frac{V^2 - V_1^2 - V_2^2}{2V_1V_2},$$

we have by making V_1 equal to V_2 and eliminating V , V_1 , and V_2 from the last two equations,

$$1 - \cos \phi = 2y \left(1 - \frac{y}{4}\right).$$

Now the power that would be given to ab (fig. 1) if there were no lag, or the apparent power, as it may be called, would be

$$\frac{V_1V_2}{r},$$

whereas the power that is actually given to ab is

$$\frac{V \cdot V_2}{r} \cos \phi.$$

Hence,

$$\frac{\text{the apparent power} - \text{the true power}}{\text{the apparent power}} = 1 - \cos \phi$$

$$\text{'' '' ''} = 2y \left(1 - \frac{y}{4}\right)$$

$$\text{'' '' ''} = 2y \text{ approximately}$$

if the lag be not very large.

For example, suppose V_1 or V_2 were 50 volts, and V were 98 volts, then y , or

$$\frac{V_1 + V_2 - V}{V_1},$$

would be 4 per cent. Hence the true power would be 8 per cent. less than the apparent power. Or, in other words, to find the true power given to ab (fig. 1), we should merely have to diminish V_1V_2/r by 8 per cent. and the answer would be obtained.

If r were unknown, and A the square root of the mean square of the current were measured instead, then to obtain the true power for the values of V_1 , V_2 , and V given above, we should diminish V_1A , the apparent power, by 8 per cent.

We will finally consider what is the percentage error made in estimating the power by the method last described, compared with the percentage error made in taking the value of $V_1 + V_2 - V$.

Let us assume that, on account of errors in the readings of V_1 , of V_2 and of V , or on account of inaccuracies in the graduation of the voltmeter, the value of $V_1 + V_2 - V$ is taken as half a volt greater than its true value, that is, that this expression is erroneously increased by 1 per cent. of V_1 if we assume V_1 to be 50 volts as above. Then y will be also increased by 1 per cent., and since the true power is obtained by subtracting from the apparent power $2y$ times the apparent power, it follows that the power measured in this way will be estimated as 2 per cent. too low if the combined error made in measuring $V_1 + V_2 - V$ be *plus* 1 per cent. of V_1 .

VI.

Measuring the Power given out by an Alternate-Current Dynamo.

In consequence of the trouble usually experienced in correctly measuring the power given to an inductive circuit, it is usual when measuring the power given out by an alternate-current dynamo to use for the outside circuits various resistances, all of which are as far as practicable non-inductive. But as the construction of adjustable non-inductive high resistances that will take large currents is a troublesome matter, we suggest the following as a convenient method of overcoming the necessity of employing such a non-inductive circuit:—

Let the circuit external to the dynamo be ac (fig. 1), only a portion of which is non-inductive; then, if V_1 , V_2 , and V have the values already given them, it is easy to show that the power given to both the inductive and non-inductive portion of, ac that is, to the whole circuit external to the dynamo, is

$$\frac{1}{2r} (V^2 - V_1^2 + V_2^2).$$

And we anticipate that, if only a small portion bc of the circuit be strictly non-inductive, this voltmeter method of measuring the power given out by an alternate-current dynamo will give more accurate

results than can be often obtained by assuming that a so-called non-inductive circuit is really non-inductive, and, therefore, that the apparent power is the true power.

IV. "On Galvano-Hysteresis. (Preliminary Notice.)" By SILVANUS P. THOMPSON, D.Sc., B.A., Professor of Physics in the City and Guilds Technical College, Finsbury. Communicated by Professor G. CAREY FOSTER, B.A., B.Sc., F.R.S. Received March 16, 1891.

1. If a sufficiently strong electric current is passed through a coil of insulated soft iron wire for a short time, and the wire then disconnected, and if, after the lapse of any length of time, the wire is placed in the circuit of a galvanometer, and is then subjected to longitudinal magnetisation or to a succession of alternately directed longitudinal magnetisations, it is found to discharge an electric current through the galvanometer.

2. The direction of the current discharged from the iron wire is found to be the same as that of the current which was originally passed through it.

3. The direction of the discharge current is opposite to that in which the discharge current would flow if the wire acted as a condenser.

4. A wire which has once produced such a discharge current will not produce a second unless again traversed by a charging current.

5. A wire which has not been subjected to any preliminary process of charging, that is to say, one which since being annealed has not been traversed by an electric current, does not sensibly show any such phenomena, either when subjected to longitudinal magnetisation or to a succession of alternate magnetisations.

6. The sense of the discharge current is quite independent of the direction of the longitudinal magnetisation used in producing the disturbance which effects the discharge.

7. The time-integral of the discharge current is independent of the duration of the charging current, provided this is not too suddenly turned off. It increases with the strength of the charging current up to a certain limit, being proportional to it through a certain range of values, but is not proportional to it for currents below or above certain limits of strength. These limits vary with the gauge of the wire, but are independent of its length. For a charging current of given strength the discharge current from a given wire is greatest if the charging current is gradually reduced to zero and not abruptly broken by a spark.

8. The time-integral of the discharge current is practically inde-

pendent of the intensity of the longitudinally-applied magnetising force if the latter exceeds a certain minimum value.

9. The author has investigated these phenomena by means of ring cores constructed of iron wire (annealed) covered with insulating material overwound with insulated copper-wire coils, the latter being wound in every case in a helix returning axially upon itself, so that the current in this copper wire should have null effect in directly generating any induced electromotive forces along the iron-wire core.

10. The effects obtained are considered by the author to be akin to those obtained by Villari,* in 1865, by the mechanical agitation of iron bars through which electric currents had been previously passed, and, like the effects of Villari, to be due to the production and subsequent disappearance of a circular magnetisation. They are also akin to those observed by Hughes† with the induction balance.

11. The author has been able to imitate and reproduce these effects by the use of copper wires immersed in iron filings, and surrounded by a magnetising coil wound so as to return axially upon itself.

Presents, April 9, 1891.

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* 'Poggendorff, Annalen,' vol. 126, 1865, p. 87.

† 'Roy. Soc. Proc.,' vol. 31, 1881, p. 531.

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April 16, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

- I. "On the Causes which produce the Phenomena of New Stars." By J. NORMAN LOCKYER, F.R.S. Received November 28, 1890.

(Abstract.)

In communications to the Society during the last four years, I have produced evidence to show that many so-called stars are composed of swarms of meteorites, and are increasing their temperature. Taking a normal case of an undisturbed swarm, I have shown, by means of a "temperature curve," the spectra given by the same mass of meteorites in its evolution from a nebula to a condensed and nearly cold body. In considering this question, the appearance of the so-called "New Stars" was referred to, and it was suggested that such appearances might be due to the collision of meteor-swarms or streams in space, an idea which I first put forward with regard to Nova Cygni in 1877.

It became obvious that a complete discussion of these phenomena would afford a valuable test of the general hypothesis, for the reason that such bodies, instead of going forward along the temperature curve, should go back as they cooled and became invisible.

All the observations have, therefore, been brought together and discussed from this point of view, the investigation having special reference to the sequence of spectroscopic changes from the first appearance of a new star to its final disappearance.

The various theories which have been put forward since the appearance of the new star of 1572 are referred to in the paper, and these are followed by a general statement of the meteoritic theory of the origin of new stars. The remaining part of the paper consists of a detailed discussion of all the observations of new stars which have been made, and the final result is a complete justification of the conclusion arrived at from the first survey that "new stars, whether seen in connexion with nebulae or not, are produced by the clash of meteor-swarms." Some of the chief points may be referred to here.

The investigation has shown that there is a close relation between the spectra of comets and the spectra of new stars, but whereas in comets only one swarm has to be considered, in new stars there are two swarms which may or may not be equally dense or of equal dimensions. The spectrum of a new star is therefore a compound one. We have, in fact, a mixed radiation and absorption spectrum similar to that presented by a variable like *Mira Ceti* when at its maximum brilliancy. In another paper I have shown that variables of the *Mira* type are really double swarms, and hence the conclusion that the difference between this class of variables and new stars is only a difference in the orbits of the subsidiary swarms.

Omitting *Nova (U) Orionis*, which proved to be only a long period variable, only three new stars have been spectroscopically observed; namely, *Nova Coronæ* (1866), *Nova Cygni* (1876-77), and *Nova Andromedæ* (1885).

In *Nova Coronæ*, when first observed, a spectrum of bright lines was superposed upon one of dark lines. The absorption phenomena were similar to those characteristic of stars like α *Orionis*, and the chief radiation was that of hydrogen. A discussion of the observations suggests that two of the ill-defined lines in the blue may have been due to carbon. In the discussion of cometary phenomena which I have previously communicated to the Society, I pointed out that in many cases the blue band appeared to have two maxima, one at λ 468 and one at λ 473, and it is more than probable that the two lines of the *Nova* were identical with those of comets.*

In comets, the blue band, whether single or double, is generally admitted to be due to carbon, from its association with the undoubted carbon band in the green, and the same origin is therefore probable in the case of the *Nova*. Whatever the origin of the two lines in *Nova Coronæ*, the fact of their being common to comets and a new star is the point I am anxious to bring out. The F line was recorded throughout the whole period of observation, and another bright line, apparently coincident with the chief nebula line, was recorded by Messrs. Stone and Carpenter.

The suggestion that a new star is produced by the collision of two meteor-swarms or streams is fully borne out by the discussion of the observations of *Nova Coronæ*. The mixed phenomena of absorption and radiation which were observed are simply and sufficiently explained on this supposition. An attempt is made in the paper to

* *Note, April 4.*—The band in question is also probably identical with the one seen in some of the stars of the Wolf-Rayet type. Dr. and Mrs. Huggins have recently made observations of some of these stars which have led them to conclude that the band is not due to carbon ('*Roy. Soc. Proc.*,' vol. 49, p. 33). I am not yet convinced on this point, but I shall take another opportunity of replying to their remarks.

show that the spectrum of the Nova can be reproduced by integrating the spectrum of a comet at a certain temperature, and a nebula of a certain degree of condensation. The resulting spectrum differs only very slightly from that of the Nova, and the differences can be accounted for by difficulties of observation.

Nova Cygni is by far the most important new star which has appeared in spectroscopic times. Numerous observations were made, and they are, on the whole, in reasonable agreement. The most complete observations were made by Vogel. When first observed, the spectrum consisted of several bright lines and flutings, the lines of hydrogen being very conspicuous. As the star gradually faded away, there was a general diminution in the number and brightness of the lines, but the most striking feature was the *brightening* of the line in the green, near λ 500, which is generally accepted to be the nebula line, as the other lines faded. Finally, the spectrum consisted solely of the line 500. The discussion indicates that, in addition to hydrogen, there was the radiation of carbon vapour, the flutings seen being those which are most frequently observed in comets. They are, however, modified by the superposition of the spectra of other substances. Practically all the lines and flutings seen in the spectrum of Nova Cygni can be explained by reference to laboratory work at low temperatures. As in the case of Nova Coronæ, the spectrum of Nova Cygni can be reproduced by integrating the spectra of bodies which we have reason to believe are swarms of meteorites. Several examples of this are given in the paper. In the earlier stages, it is necessary to integrate the spectra of at least three swarms of different degrees of condensation, but as the spectrum became simpler, two are sufficient. The compound origin and character of the spectrum of a Nova is thus clearly indicated. It is not to be supposed from these integrations that in the first instance there are really three or more swarms engaged. A Nova is probably produced by the collision of only two swarms, but the resulting mixed swarm is so complicated that we can only represent it by assuming at least three temperature conditions. There will be the temperatures corresponding to each of the central condensations, and that corresponding to the outliers. As the swarm cools, the temperature becomes more equal throughout, and finally the swarm resembles a planetary nebula.

The spectrum of Nova Andromedæ was at no time a very striking one, and was always difficult to observe. It was also further complicated by being superposed upon the then imperfectly recognised spectrum of the Great Nebula, in which it was involved. The spectrum was almost continuous, with brighter portions here and there, which could only be measured with difficulty. Consequently, the results obtained by different observers are somewhat discordant. The discussion shows that what was really observed after the star had

faded was nothing more than the spectrum of the nebula itself, as might be expected. Owing to the difficulty of making the observations, the apparent variations of the spectrum from day to day may not be real, and it is hopeless to attempt to explain them by a reference to the effects produced by a gradual fall of temperature. As the star only fell two magnitudes during the whole period of spectroscopic observation, the change of temperature would not be so great as in Nova Cygni, and the variations would not be so well marked. No lines or bands, however, were on any occasion recorded in the spectrum with which we are not familiar in other bodies which, there is evidence to show, are meteoritic swarms. A diagram shows that the spectrum of the Nova, as seen by Copeland, on October 1, can be reproduced by adding the spectrum of hydrogen to that of the nebula.

It is next pointed out that the theoretical sequence of phenomena in the spectrum of a Nova produced by the collision of two swarms of different densities is in strict accordance with the partial sequences actually observed.

A discussion of the colour phenomena shows also that in Novæ we have to deal with mixed swarms, the colours at certain stages being compound ones.

In my former paper, I have shown that carbon radiation is one of the chief characteristics of uncondensed meteor-swarms, and the discussion of the new stars has revealed the fact that carbon is also one of the chief characteristics of their spectra, though modified by other substances.

The observed changes in magnitudes of Novæ are also in accordance with the collision theory. The rapid fading away demonstrates most conclusively that small bodies, and not large ones, are in question.

The observations with which I have had to deal have often been imperfect, owing to the difficulty of observing this class of bodies, and different observers have frequently disagreed with regard to some of the spectroscopic details, but still, as I have endeavoured to show, most of the discrepancies can be reconciled when difficulties of observation are allowed for.

- II. "An Attempt to determine the Adiabatic Relations of Ethyl Oxide. Part I. Gaseous Ether." By W. RAMSAY, F.R.S., Professor of Chemistry in University College, London, and E. P. PERMAN, B.Sc. Received March 16, 1891.

(Abstract.)

The object of the research described in the memoir is the determination of the behaviour of ether in the state of gas approaching towards the state of liquid, when heat is communicated to it, so as to alter its condition adiabatically.

Previous researches by one of the authors in conjunction with Dr. Sydney Young have yielded data regarding the relations of pressure, temperature, and volume of gaseous and of liquid ether from which the values of the isobaric and of the isochoric differentials are obtainable. Such results lead directly to a knowledge of the differences between the specific heats at constant pressure and those at constant volume; and these differences are not constant, but vary with varying volume, pressure, and temperature.

The memoir contains an account of experiments made to determine the ratio between the specific heats at constant pressure and those at constant volume. The velocity of sound in gaseous ether was determined at various temperatures, pressures, and volumes; and by means of the isothermal differentials, and the experimental results for the velocity of sound, the ratios between the two specific heats were calculated. From the differences and the ratios of the specific heats, the values of the specific heats were deduced.

The general conclusion is that, for any constant volume, the specific heat, whether at constant volume or at constant pressure, decreases to a limiting value with rise of temperature, and subsequently increases; and that the change with temperature is more rapid, the smaller the volume.

At large volumes, the specific heats tend towards independence of temperature and volume, while at small volumes, the influence of change of temperature and volume is very great.

The authors are at present investigating similar relations for liquid ether.

III. "On the Physical Characters of the Lines in the Spark Spectra of the Elements." By W. N. HARTLEY, F.R.S., Professor of Chemistry, Royal College of Science, Dublin. Received March 18, 1891.

The properties of the atoms are a periodic function of their masses, and the physical characteristics of the spectra of the elements appear to be an expression of the properties of the atoms; for there is undoubtedly an intimate connexion between the rays emitted by the self-luminous vapours of the elements and their chemical and physical properties.

If we photograph the spark spectra of thirty or forty of the elements and arrange the spectra in groups following the periodic law, the arrangement will be seen to be a perfectly natural one. This observation applies not only to the groupings of the lines, but also to the physical characteristics of the individual lines. In spark spectra, the three most striking characteristics are (1) an extension of certain lines above and below that part of the spectrum bounded by the points of the two electrodes; (2) the nimbus which surrounds the extremities of the lines, even to some extent those portions which form an extension; and (3) the continuous spectrum which forms the background to the lines.

(1.) *On the Extension of the Lines.*—The spark discharge, as shown by Perrot, is composed of two parts, of which the fiery track, or central portion, is a statical discharge, and the aureole, or flame, is dynamical, and capable of electrolytic action.

From careful observation of the sparks, and photographs of spectra, I have come to regard all those spectra with lines extended as spectra of different discharges taken simultaneously. The principal lines lying between point and point of the electrodes are spectra of the fiery path of the spark; the extension of the principal lines above and below the points of the electrode appear to be spectra of the aureole. The principal observation which leads to this conclusion is that the electrodes are seen to glow silently and continuously above and below the points of the upper and lower electrodes, and frequently slight roughnesses present the appearance of brightly but steadily shining dots; particularly is this the case with those metals which exhibit the most extended lines, as for instance, cadmium, thallium, and indium. The lines in many spectra are free from this extension, and no glow is observed on the electrodes. A study of about thirty different spectra of the metals and semi-metallic substances has led to the following observation.

Elements which are difficult to volatilise, and those which are bad conductors of electricity, do not exhibit spectra with extended lines; and, con-

versely, metals which are the best conductors and the most volatile exhibit spectra with their principal lines largely extended.

The following metals are good conductors, that is to say, sufficiently good not to impede the spark when broad electrodes are used, and they are more or less volatile. They show a large extension of their principal lines :—

	Boiling point.	Atomic mass.		Volatility.	Atomic mass.
Magnesium..	1100° C.	24·4	Aluminium..	Not volatilised by ordinary means.	27·08
Zinc	924° to 954° C.	65·3	Indium	Volatilised at a red heat.	113·7
Cadmium ...	763° to 772° C.	112·1	Thallium....	Easily volatilised at a red heat.	204·2

	Atomic mass.
Copper	Not volatilised by ordinary means.. 63·33
Silver.....	Boils about 1570° C. 107·93
Mercury.....	„ 357° C. 200·1

In these examples the extension of the lines is least in the case of the least volatile metals, which are also those of least atomic mass, and it is greatest with those which are most volatile and of greatest atomic mass.

The continuous spectrum in these examples is very weak, and the air lines are almost absent from the thallium and mercury spectra, the air spectra being suppressed by the excess of dense vapour in the track of the spark. The lines most extended are the following :—In

the cadmium spectrum, those with wave-lengths $\overbrace{3611\cdot8, 3609\cdot6}$ (*a pair*), $\overbrace{3466\cdot8, 3465\cdot4}$ (*a pair*). These pairs appear as single lines if the dispersion is insufficient and the definition imperfect.

The most refrangible line of each pair is the more extended. The other lines in this spectrum are 3402·9, 2747·7, 2572·2, 2313·6, and 2265·9, all with fine extensions. In the spectrum of thallium, wave-lengths 3775·6, 3528·8, 3518·6, and 2917·7.

In the spectrum of mercury, the lines with wave-lengths 4358, 4046·5, and 3984 are well extended, but the most important extensions in this spectrum are the lines with wave-lengths 3662·9, 3654·4, 3632·9; the last of these, which form a well-marked triplet, is by far the most extended. The pair of lines 3130·4 and 3124·5 are greatly extended, and the same remark applies to 2966·4 and 2946·6.

The dimensions of the principal lines in the cadmium, thallium,

and mercury spectra were measured on my enlargements. The principal portion of the lines lying between point and point of the electrode was 42 mm. in all spectra. The extension of the lines *below* was 22 mm. to 25 mm., extension *above*, 9 mm. to 10 mm. As the extension is always sharp and well defined, it is an important feature in these spectra. Even concentrated solutions of the metals, when photographed with graphite electrodes, exhibit this extension in their principal lines. For instance a solution of beryllium chloride shows a very remarkable extension above and below the points of the upper and lower electrodes; the dimensions of the principal line, wavelength 3130·2, are as follows: between the points, 42 mm.; *below*, 10·5 mm.; *above*, 17·5 mm. It is at the upper or positive electrode that the longest extension is observed, but at the lower or negative electrode that it is strongest. In the case of the cadmium lines, the extension is smaller, but strong at the side of the negative electrode, and very fine and long at that of the positive.* The appearance of lines due to impurities or traces of metals in the spectrum of the negative electrode only, I have attributed to the oscillation of the spark discharge, and the fact that the negative electrode is the hotter.†

(2.) *The Nimbus*.—The nimbus is not apparently dependent on the volatility or the oxidisability of the vapour of the elements, though these properties are connected therewith.

By far the largest nimbus is that of magnesium; those of cadmium and mercury stand next in order; the smallest are those of platinum, gold, copper, and silver. It is thus evident that neither conductivity nor vapour density controls it, for there is very little nimbus on the lines of the thallium and iridium spectra; but volatility certainly increases it. There is a considerable nimbus on some of the lines in the spectra of arsenic, antimony, and bismuth; also on a few lines of tin and of lead. In the case of magnesium, the cause of the dense and large nimbus is probably the intensity of the chemical action of which the rays of the incandescent vapour are capable, together with the large quantity of metal in the track of the spark, owing to its volatility.

The chemical activity of the zinc rays is less than that of the rays of magnesium, but the effect of this is overbalanced by the density of the vapour and the volatility of the metal being both greater; accordingly the lines of zinc have a large nimbus. The nimbus is somewhat larger on the lines of cadmium than on those of zinc, the volatility and the density of the vapour are both greater.

* In a paper published in the 'Scientific Proceedings of the Royal Dublin Society,' on the constitution of electric sparks, this does not appear in the lithographed illustration, but I have carefully verified the fact by referring to the original photographs.

† *Loc. cit.*, p. 373.

The nimbus is evidently an expression of the quantity of matter in the spark, and the intensity of the chemical action which the rays emitted by its ignited vapour are capable of exerting.

(3.) *On the Continuous Spectrum which forms the Background to the Lines of certain Spectra.*—This must be caused by the ignition either of some solid substance or of a vapour which is not that of an element but an oxide. An examination of the spectra in which the continuous background of rays is a conspicuous feature discloses the fact that the metals which are not oxidisable do not possess it, for instance, gold, silver, and platinum. Metals of the iron group show it near the points of the electrodes when the non-volatile oxides are formed. The very volatile metals with volatile oxides, such as mercury, iridium, thallium, zinc, and cadmium, do not show it.

Spectra of the metalloids, such as tellurium, arsenic, antimony, and bismuth, which are not only volatile but which form volatile oxides, show it very strongly. Ordinarily, magnesium does not show it, because the exposure necessary for photographing the spectrum of that element is less by one-half the period of the others, and by one-quarter that of tellurium. When a plate is long exposed to the rays of magnesium, the continuous spectrum appears at the points of the electrodes where the non-volatile oxide would be formed. It may be considered that in the passage of the spark, the vapour of the element fills the track, and this vapour, on cooling, forms, for a minute period of time, an incandescent oxide, and, the spectrum of this being a continuous spectrum, its photograph appears as a background to the rays emitted by the element.

But it is nevertheless the fact that the continuous background is a very characteristic feature of the metalloids, though why the vapours of these oxides should produce this action more conspicuously than those of the oxides of the volatile metals, there seems to be no sufficient or well-understood reason to be advanced at present. It may be that the vapours of the metalloids in cooling emit a continuous spectrum for a short period prior to oxidation.

On the Breadth of Lines.—It is well known that, under identical conditions, the principal lines in the spectrum of an element become stronger and broader as the rays forming the spectrum proceed from a larger quantity of material, that is to say, form a denser radiating layer. It is evident, then, that in any series of three or more elements of similar character, the intensity and the breadth of the lines in their spectra will depend upon (1) intensity of chemical energy, (2) volatility and vapour density, and (3) electric conductivity of the metal.

In accordance with these conditions, the lines of cadmium are broader than those of zinc, and the lines of zinc broader than those of magnesium.

Presents, April 16, 1891.

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Phototype Portrait of Horace Benedict de Saussure.

M. Henri de Saussure

April 23, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Contributions to the Chemical Bacteriology of Sewage."

By Sir HENRY E. ROSCOE, F.R.S., D.C.L., LL.D., and
JOSEPH LUNT, B.Sc., F.C.S. Received April 23, 1891.

(Abstract.)

The present research contains the results of experiments on the chemical and bacteriological examination of sewage micro-organisms, made with the object, in the first place, of ascertaining what species are there present, and, in the second, of determining some of their chemical characteristics.

The authors have isolated from crude sewage, by methods which are fully described, a number of organisms which may serve as typical examples of those usually present in this material. Some of these have already been described, whilst others are believed to be new organisms.

The microscopic and macroscopic appearances of the organisms and their pure cultures have been carefully recorded by means of photographs, which give in a permanent form their morphological characters and the plate- and tube-cultivations in their most characteristic stages of growth. This method of illustration the authors consider to be of much importance, as bacteriological descriptions of organisms are frequently of little value for the want of accurate representations of the microscopic preparations and pure cultures.

The experiments described were undertaken with the object of studying the reactions of sewage organisms from a chemical point of view, and of gaining information as to the rationale, both chemical

and bacteriological, of the two marked changes which sewage is liable to undergo, i.e., on the one hand purification, or the gradual destruction of putrescible matter without the formation of offensively smelling products, and on the other putrefaction. It was desired to ascertain which organisms are concerned in the first of these processes and which in the second, as likewise to gain an insight into the methods by which such changes are effected.

For all the organisms described, the authors have determined the absorptive power for free oxygen when cultivated in a perfectly pure state, and also for which of the organisms free oxygen is a necessity of their activity and growth.

Each organism has been examined as to its power of growth in a liquid medium from which every trace of free oxygen, both gaseous and dissolved, has been rigorously excluded.

It is shown that anaërobic organisms associated with putrefaction, although able to grow in complete absence of oxygen, yet when that gas is present are able to *absorb it rapidly*, and thus prepare the conditions for their anaërobic growth.

The following methods for the isolation of micro-organisms have been used:—

- (1.) The method of gelatine plate-culture.
- (2.) A method, fully described, for the isolation and cultivation of anaërobic organisms.
- (3.) A method for the isolation of spore-forming organisms.
- (4.) The dilution method.

The method used for the isolation of anaërobic organisms consists in their cultivation in a specially devised form of flask containing sterile nutrient broth, through which liquid could be passed a stream or pure hydrogen, freed from all traces of oxygen by passing over glass beads, in two Emmerling's tubes, moistened with alkaline pyrogallate.

As the authors have shown in a previous paper ('Chem. Soc. Journ.,' 1889, Trans., p. 554), this treatment frees the liquid completely from dissolved oxygen.

Crude sewage was carried through three cultivations in pure hydrogen, when it was found that not only had all aërobic organisms been eliminated, but only one form of anaërobic organism appeared, viz., *Proteus vulgaris*, and this method may be used for its isolation. Several other organisms, although isolated by different methods to the above, were found to grow in the pure state in nutrient broth from which all traces of free oxygen had been excluded. These are fully described in the paper.

In the method for the isolation of spore-forming organisms, all others were eliminated by heating the sterile broth, in which a sowing had been made from crude sewage, to 80° C. for ten minutes.

The still living spores were then further isolated by plate cultivation, either with or without previous incubation of the broth tube.

For the purpose of studying the absorptive power for free oxygen, pure cultures were sown in sealed flasks with two necks, containing 25 c.c. of nutrient broth and 250 c.c. of air. These were incubated at 20—23° C. for seven days, after which time the flasks were opened and the gases remaining abstracted for analysis. It was seen that the various organisms exhibited great differences in their absorptive power for free oxygen, some showing the feeblest absorption, whilst others abstracted nearly every trace of oxygen from an atmosphere ten times as large as the culture liquid during seven days' incubation.

The rate of absorption of *dissolved* oxygen was also determined for a number of the organisms by sowing tap-water aerated under known conditions, and containing a definite amount of dissolved oxygen, with 1 per cent. of a pure broth culture of the organism which had been incubated for two days after sowing. It is shown, in the case of those organisms which absorb oxygen rapidly from the air, that the water is completely de-aerated in fourteen hours.

It is shown that certain organisms which are capable of growing in an atmosphere devoid of oxygen, *i.e.*, anaërobic, are yet incapable of liquefying gelatine without the presence of that element, although when grown in air such liquefaction is extremely rapid.

Cultivations were made in the form of flask referred to for anaërobic organisms, in which the organisms were sown in molten gelatine, through which pure hydrogen was passed for half an hour. The flask was then sealed. After five days' incubation, no liquefaction whatever took place, although, when exposed to air, the normal rapid liquefaction of the gelatine afterwards occurred.

It is also shown, both in the case of aërobic and anaërobic organisms, that a very appreciable diminution of the liquefying power of organisms takes place after repeated sub-cultivation in nutrient gelatine.

The method employed for photographing the micro-organisms is also described. In all cases the bacteria were stained with methyl violet, but, as this stain transmits chemically active rays, it was necessary, in order to obtain actinic contrast, to use a coloured screen and isochromatic plates. The screen adopted (a weak solution of potassium bichromate) was spectroscopically adjusted to the stain employed, so that the objects appeared black on a bright yellow background. The apparatus employed was of the simplest kind, and the source of illumination was a common duplex paraffin lamp.

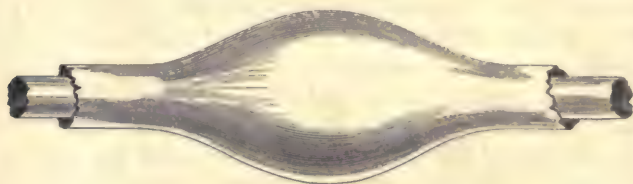
The organisms isolated from the sewage under examination are described and illustrated photographically, as regards microscopic preparations and plate- and tube-cultures.

II. "Note on the Instability of India-rubber Tubes and Balloons when distended by Fluid Pressure." By A. MALLOCK. Communicated by LORD RAYLEIGH, Sec. R.S. Received March 16, 1891.

When an india-rubber tube is expanded by internal fluid pressure, it preserves its cylindrical form until the increase in its diameter bears a certain proportion to its diameter when unstrained; but, when more fluid is introduced, the condition of the tube becomes unstable, and the internal fluid pressure diminishes.

When more fluid, therefore, is introduced into a length of tube than will suffice to expand it to its stable limit, it no longer remains cylindrical throughout its length, but assumes the form of a cylinder with one or more bulbous expansions; and the diameter of the part which remains cylindrical, though greater, of course, than the unstrained diameter, is less than that attained at the stable limit in fig. 1.

FIG. 1.



In the case of an elastic hollow sphere, although the spherical form is retained, whatever be the amount of fluid introduced, there is a similar limit to the pressure which the elastic reaction of its walls can cause within it.

If the thickness of the walls of the tube or sphere is small compared with the radius, and if, further, the material of which they are composed be considered as incompressible, while the other elastic constants are invariable for such extensions as are involved (assumptions which are approximately true for india-rubber), the value of the radius when instability begins, may readily be found. Taking δx , δy , δz as the sides of any small cube of the material of the walls, δx and δy being parallel to the tangent plane of the surface and δz normal to it, let a stretching force act in the direction of x causing δx to become $p \delta x$.

Since the material is incompressible, δy and δz , under the influence of this force, will become respectively $(1/\sqrt{p})\delta y$ and $(1/\sqrt{p})\delta z$.

Now, maintaining this force, let another stretching force act in the direction of y , which would, if acting alone, stretch δy to $\kappa \delta y$.

Then $p \delta x$ will become $\frac{p}{\sqrt{\kappa}} \delta x$;

$$\frac{\delta y}{\sqrt{p}} \quad , \quad \frac{\kappa}{\sqrt{p}} \delta y ;$$

$$\frac{\delta z}{\sqrt{p}} \quad , \quad \frac{1}{\sqrt{(\kappa p)}} \delta z .$$

The force required to stretch δx to $p \delta x$ is

$$\delta F_x = q \frac{p-1}{p} \delta y \delta z ,$$

and that required to stretch $\frac{\delta y}{\sqrt{p}}$ to $\frac{\kappa}{\sqrt{p}} \delta y$ is

$$\delta F_y = q \frac{\kappa-1}{\kappa} \sqrt{p} \delta x \delta z ,$$

where q is Young's modulus for the material.

In the case of the cylinder, if x be taken parallel to the axis of the cylinder and y round its circumference—

$$\begin{aligned} \int \delta x &= \text{unstrained length} && \text{of cylinder} = l_0 ; \\ \int \delta y &= \quad , \quad \text{circumference} && , \quad = 2 \pi r_0 ; \\ \int \delta z &= \quad , \quad \text{thickness} && , \quad = t_0 ; \end{aligned}$$

hence the whole elastic circumferential stress is

$$F_y = q \frac{\kappa-1}{\kappa} \sqrt{p} l_0 t_0 ,$$

and the fluid pressure, P , due to this stress is

$$\begin{aligned} P &= \frac{q \frac{\kappa-1}{\kappa} \sqrt{p} l_0 t_0}{\frac{y_0}{2 \pi} \frac{\kappa}{\sqrt{p}} \frac{p}{\sqrt{\kappa}} l_0} \\ &= \frac{2 \pi q t_0}{y_0} \frac{\kappa-1}{\kappa \sqrt{\kappa}} \\ &= q \frac{t_0}{r_0} \frac{\kappa-1}{\kappa \sqrt{\kappa}} . \end{aligned}$$

This is a maximum when $\kappa = 3$.

Since P is also equal to $F_r \div$ strained area of base of cylinder,

$$P = \frac{q \frac{p-1}{p} t_0 y_0}{\frac{y_0^2 \kappa^2}{4\pi p}},$$

and by equating this to the former expression, we have for p in terms of κ ,

$$p = \frac{\sqrt{\kappa(\kappa-1)} + 2}{2}.$$

So that, when $\kappa = 3$, $p = \sqrt{3} + 1$.

From this it will be found that the critical value of the radius is

$$1.815 r_0,$$

and that then the length of the tube is $1.58 l_0$ nearly.

In the case of the sphere the maximum pressure will also be attained when $\kappa = 3$, but, since by symmetry p now $= \kappa$, we shall have for the critical value of the radius $r_0 \sqrt{3}$, or $1.73 r_0$ nearly.

Some experiments were made with india-rubber pipes and balloons to see how nearly their behaviour conformed to the theory just given.

Fig. 2 (p. 461) shows the apparatus employed.

The india-rubber to be experimented on was placed in a large closed vessel, B, full of water. Two pipes C and D passed through the stopper of B; of these C communicated with the interior of the experimental tube or sphere A, and D immediately with the contents of B. A pressure gauge was connected with C.

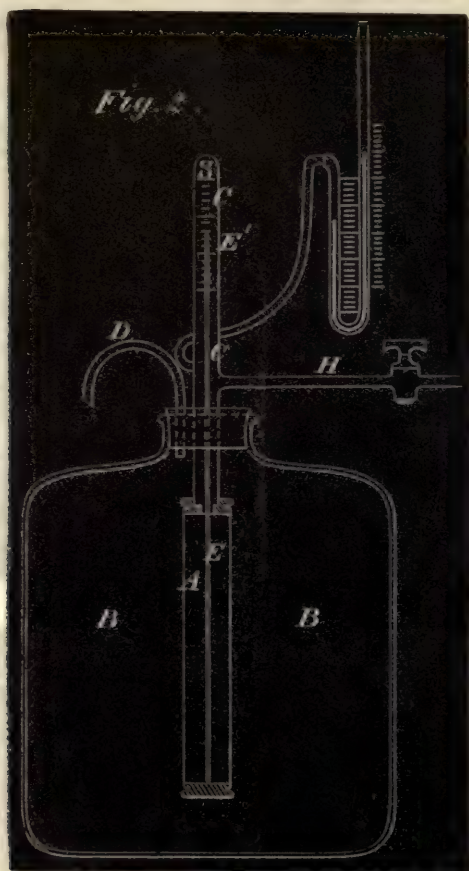
When tubes were being experimented on, the ends were closed with hard wood discs, covered with paraffin, through the upper one of which C entered. Fixed into the centre of the lower disc was a long straight wire, E, which passed freely through C, and the position of whose upper end, E', could be read on the scale S.

Water could be introduced into A by means of the pipe H connected with C.

When every part of the apparatus was filled with water, and the pressure gauge showed that the internal and external pressures on the india-rubber were equal, more water was admitted through C. The volume of water thus introduced was measured by the amount expelled through D. The pressure gauge showed the internal pressure in A, and the descent of E' gave the elongation of the tube.

The analysis of the results thus obtained is given by the curves in Diagram I.

The experiments on spheres were made in the same way, except that the wire E was not used.



The results are given in Diagram II.

Diagram III gives the values of the function $(\kappa-1)/\kappa\sqrt{\kappa}$ in terms of κ .

In Diagram I the abscissa is r/r_0 .

(a) shows the observed pressure in the tube.

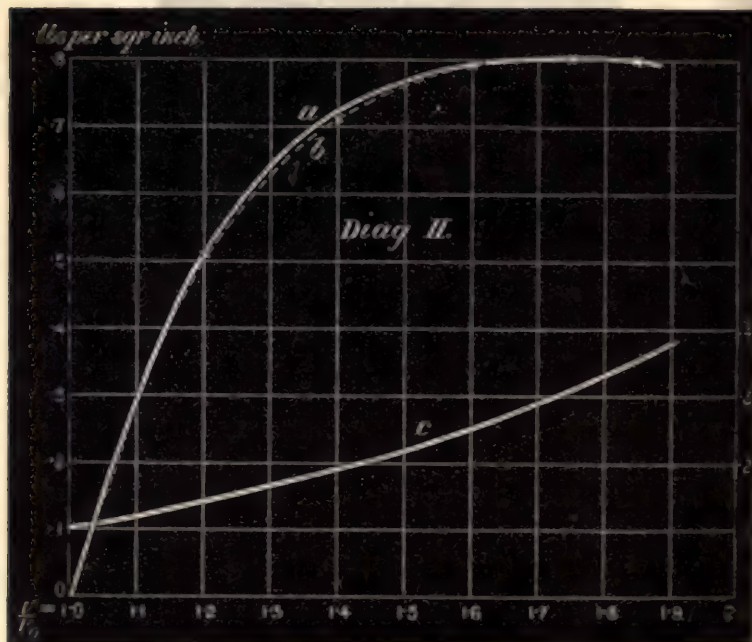
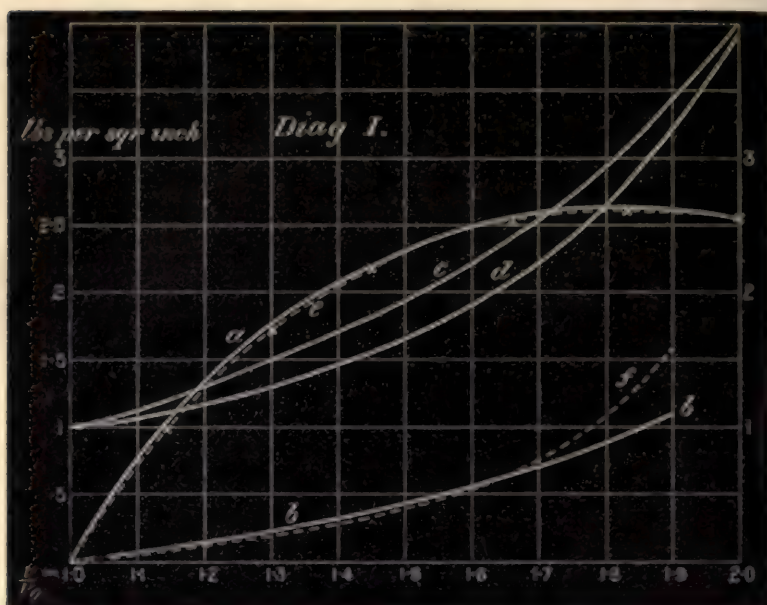
(b) „ „ extension of „

(c) } represent the values of $\left\{ \begin{array}{l} \kappa \\ p \end{array} \right.$

(e) is the theoretical pressure.

(f) „ „ extension.

After the unstable state is reached the formula for the extension does not apply. For this tube $t_0/r_0 = 0.039$.





In Diagram II—

(a) shows the observed pressure,

(b) „ „ theoretical pressure,

in an india-rubber balloon for which $t_0/r_0 = 0.0125$.

(c) is the value of κ .

The rather uncertain nature of the measurements of both t_0 and ν_0 in these experiments makes the close apparent agreement between the observed and theoretical results somewhat illusory; but it shows at any rate that, if among the values obtained for t_0 and ν_0 those are taken which make theory and observation coincide for one value of P , the remaining observations will also lie on the theoretical curve.

Presents, April 23, 1891.

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April 30, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

- I. "Cloud Photography conducted under the Meteorological Council at the Kew Observatory." By Lieut.-General R. STRACHEY, R.E., F.R.S., and G. M. WHIPPLE, Superintendent of the Observatory. Received April 23, 1891.

In 1878 the Meteorological Council decided upon undertaking a series of experiments with the view of attempting by means of photography to obtain a record of the height and velocity of the clouds, as indicating the movements of the upper parts of the atmosphere. For this purpose a plain cubical camera was constructed, with its optical axis directed to the zenith, and a number of pictures of clouds were thus obtained. The results were so far satisfactory as to establish the possibility of identifying points in the clouds which would admit of the calculation of their height with considerable precision. But, owing to the small field of view of the lens made use of, it was found that the opportunities of photographing clouds in this manner were of somewhat rare occurrence, and it was therefore decided, on the proposal of Captain Abney, to whom the Meteorological Council is indebted for his valuable advice throughout the course of these experiments, to construct two cameras so arranged as to enable them to be directed to any part of the sky, and thus to photograph clouds in all positions.

For this purpose the cameras were fitted with theodolite mountings, provided with altitude and azimuth circles. The dark slides for carrying the sensitised plates were fitted with glass plates, upon which cross lines indicating the position of the optical axis were etched. These lines were photographed simultaneously with the clouds, and the readings of the divided circles, recorded at the time of exposure, thus supplied the altitude and azimuth of the point of the cloud covered by the intersection of the cross lines at that moment.

From a photographic picture of a series of staves erected at known angular intervals, a scale of angular distances was obtained, by means

of which the azimuth and altitude of any point in the cloud picture could be deduced from those of the intersection of the cross lines.

Arrangements were made for erecting these cameras at the extremities of a base of known length (800 yards), between which an electrical communication was established.

Spring shutters were placed over the lenses, which could be liberated and again closed, at the will of the observer, by the passage of an electric current, so as to expose the plates for any desired interval of time.

Captain Abney also, after numerous trials, devised a suitable formula for an emulsion for coating the plates, as special precautions were found to be necessary in order to obtain good cloud photographs.

Captain Abney thus describes the photographic process he proposed:—"My attention has been once more directed to the best photographic process to employ for the delineation of the clouds, a certain inconvenience having attached to the use of collodion-emulsion, which at first I had not foreseen. I had then recourse to gelatine plates, but the manner in which they are ordinarily prepared induces a sensitiveness which becomes unmanageable, even when a diaphragm with a small aperture is used in the lenses. The great desideratum in the plates appears to be that a small variation in the intensity of the light proceeding from the sky or cloud shall produce a great contrast in the intensity of the developed image. A very rapid plate does not answer for this purpose; hence I tried several modifications. The process which at present has given the best results is as follows:—

"150 grains of bromide of ammonium and 10 grains of iodide of potassium are dissolved in 3 oz. of water, to which 80 grains of Nelson's No. 1 photographic gelatine and 80 grains of Coignet's gelatine have been added. This is dissolved by the aid of heat, and 200 grains of silver nitrate dissolved in $1\frac{1}{2}$ oz. of water are added. The whole is warmed to 100° F. for five minutes, and allowed to set after being poured out in a flat dish. The emulsion thus produced is washed (in the usual manner) from the soluble salts, and is then re-melted and plates coated and dried, as is customary in the gelatine process.

"This formula gives very constant results, and great contrasts of image are obtained by careful development."

The years 1881 to 1884 were passed in working out the details of the arrangements above described, and in 1885, after numerous preliminary trials, it was resolved to erect the two cameras at the Kew Observatory. One was placed on the roof of the Observatory building, and the other on a stand in the Old Deer Park, 800 yards from the other, on the road leading to the Observatory from Richmond; and a telegraph cable carrying two insulated copper wires of

low resistance, buried a few inches below the surface of the ground, was laid between the two stands. Switches, attached to telephones as well as to an electric battery, were fixed to these stands, and wires were arranged on the cameras, so that the observers could either communicate with one another, or work the exposing shutters of the two cameras at will.

Operations for the determination of cloud height and motion were then carried out on suitable occasions, as follows:—The two observers, termed for convenience A and B, proceeded to their respective stations, each provided with a box containing half-a-dozen dark slides charged with sensitised plates, and also an adjusted watch. The cameras were set up on the pedestals, levelled, and the connecting wires joined up. Locking plates of peculiar construction were provided, which ensured that the zero points in azimuth of both cameras were exactly directed to the same point of the horizon.

The observer at A, when he saw B had reached his station and placed his camera on the pedestal ready for use, attracted B's attention by means of a flag waved overhead, and directed him through the telephone to set the instantaneous shutter of his camera, setting that of his own camera at A at the same time. A then, making use of the push, sent a current of electricity through the two cameras, which should liberate both shutters at the same instant of time. An enquiry was immediately made through the telephone of B, and, if the reply assured A that the shutters were working satisfactorily, the observers proceeded to the second stage of the observation, which was as follows:—

A carefully examined the sky and, selecting a suitable cloud, directed the sights on his camera towards it, making a convenient setting of the horizontal and vertical circles, which he then read off. He then told B to set his camera to the same azimuth and altitude, and insert a loaded plate-holder in its groove, repeating the circle readings to ensure accuracy, and also at the same time to set his shutter. A, whilst directing B through the telephone, conducted the same series of operations at his own instrument, so that, as soon as B telephoned that he was ready for action, A switched the battery on to the line, and, watching the cloud for a favourable instant, touched the push, whereby the two plates were exposed simultaneously, the instant of the exposure being recorded by both observers in their respective note-books. They then quickly exchanged their plate-holders for others containing fresh plates, and again set the shutters, so that by the time sixty or seventy seconds had elapsed since the first exposure was made they were ready for a second, which was carried out as before under the directions of A, both observers again noting the time. After this, A, having switched on the telephones, enquired of B if he had obtained the two pictures. If the reply was in the affirmative,

he was directed to read both his circles, and to enter the readings, with the times of the two exposures and the numbers of the plate-holders in his book, A doing the same for his own instrument.

Having deposited the plate-holders in the light-tight carrying box, another charged pair were taken, and a fresh cloud in another part of the sky selected, and the operations already detailed were repeated, until the stock of charged holders was exhausted.

The observers then, by means of the telephones, again compared their watches, and noting their differences, if any, sighted their cameras on each other, and read their mutual bearings and altitudes. This was done in order to be sure no displacement had taken place in either the orientation or level of the instruments. They then unlocked the stands, dismounted the cameras, and put them away in the lockers of the pedestals, ready for use on another occasion, conveying the plates to the photographic laboratory for development and subsequent treatment.

From time to time, the empty plate-holders were taken out, the lenses directed to each other, and settings made and circles read with the view of determining the true bearings of the fiducial lines before described, from which the angular position of the cloud-points dealt with were obtained.

On removal of the exposed plates from the holders, the dates of the observation having been written on each of the films in pencil, as well as a register number, development proceeded. This was conducted in a wooden tray with a glass bottom specially adapted to hold four plates. The two A's and two B's forming one set of pictures were usually selected for simultaneous development, in order that the negatives obtained might possess the same degree of intensity. Before hydrokinone became an article of commerce, a solution of pyrogallic acid or sulphate of iron was employed as the developing agent, but, since 1889, Edwards's hydrokinone developer has been employed by preference, as being less liable to produce fogged plates.

Owing to the efforts of the Kew observers being chiefly directed to photographing high cirrus clouds, very careful and slow development was required, to produce satisfactory negatives, and it has been generally necessary to continue the operation for about forty minutes to bring out a successful result. In some cases of very thin filmy cirrus, the so-called mare's tail clouds, the development occupied $1\frac{1}{2}$ hours, before the picture appeared.

For discussion of the photographs, in most cases prints were made of the negatives by the ordinary albuminised paper process.

Various methods of obtaining the heights and velocity of motion of the clouds from the photographs thus made have been attempted. The computation by the ordinary trigonometrical formulæ from the

azimuths and altitudes derived by measurement of a series of points in the clouds, properly identified in the sets of pictures, is very tedious, and a graphical method was suggested by Sir G. Stokes, which, though very ingenious, was found to be troublesome in practice, and was not persevered in.

From the nature of the process employed, the indefinite outlines of the clouds, and their incessant change of form, complicated by the effects of perspective distortion on an irregular and ill-defined surface, it is necessarily impossible to identify cloud-points in the different pictures with much precision or make exact measurements; and approximate results, therefore, are all that can be sought for. The object of the enquiry is chiefly to determine the velocity of movement of clouds at varying heights above the earth's surface and to obtain the heights of those observed at the greatest elevations, which appear as cirrus.

If A and B are the azimuths of any point in a cloud, and Z_a and Z_b the zenith distances, observed respectively at A and B , the ends of the base β , then the distances, measured in a horizontal plane passing through the base, D_a , D_b from A and B respectively of the point vertically under the cloud-point will be

$$D_a = \beta \frac{\sin(B)}{\sin(A-B)}, \quad D_b = \beta \frac{\sin(A)}{\sin(A-B)},$$

and H , the height of the cloud-point above the horizontal plane passing through the base, will be

$$H = \beta \frac{\sin(B)}{\sin(A-B) \tan Z_a} = \beta \frac{\sin(A)}{\sin(A-B) \tan Z_b}.$$

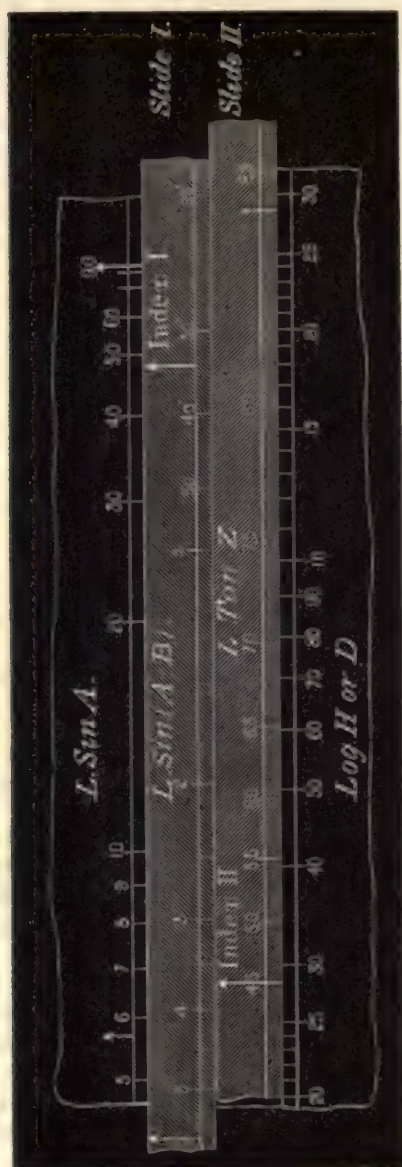
These values are readily found by means of a slide-rule constructed as shown below. The graduations of the upper scale of the fixed rule are log sines; those of the lower scale of the fixed rule logs of numbers, the log of 2400 feet, the length of the base, coinciding with log sin 90° .

The upper sliding rule No. I is graduated with log sines of small angles on the same scale as the first rule, the point marked with index No. I indicating log sine $5^\circ 44' 27''$, which is 9.00000, or $0^\circ 34' 23''$, which is 8.00000.

The lower sliding rule No. II is graduated with log tangents Z , the point marked with index No. II, corresponding to log tan 45° , and on the same scale as the sines.

To apply the rule, bring index No. I of the slide-rule No. I opposite the angle A on the upper fixed scale. Then bring the index No. II of the slide-rule No. II opposite to the angle $A-B$ on the slide-rule No. I.

FIG. 1.



Opposite the index No. II, or $\tan 45^\circ$, will be found on the lower fixed scale the distance D_b , in feet; and opposite to the angle Z_b will be found on the same scale the height of the cloud in feet. By a

similar process will be found the distance D_a and a height of the cloud determined from Z_a .

The position of the point vertically under the selected cloud-point will be determined with sufficient accuracy graphically, by the intersection of the two distances measured from the ends of a line drawn to represent the base.

The repetition of this process for the second set of photographs will in like manner give the position of the cloud-point after the interval elapsed between the taking of the two sets of pictures, and the distance travelled being measured on the diagram, the velocity can be found, and the direction of motion will be shown in relation to the direction of the base.

Irrespective of the laborious nature of this process, it was found that the angles on which it was based were often so small that the results obtained were inconsistent and unreliable.

In 1890, therefore, it was decided to try another method of observing, which would admit of much simpler treatment. This was to fix the cameras so that the optical axes were directed to the zenith, and to photograph clouds which passed across the field of view which is comprised within a circle described at an angular distance of about 15° round the zeniths of the two stations. The defect of this method is that it very materially limited the scope of operations, and reduced the opportunities of taking pictures to a comparatively small number, for it was found that a large proportion of the clouds which seemed apparently favourable for photographing when viewed by reflected solar light incident upon them at oblique angles became almost invisible when observed directly overhead. This was notably the case with cirrus, some forms of which, especially those possessing the nature of cirro-stratus, appear as practically structureless masses when seen in this position. But notwithstanding these drawbacks, some of which, it is hoped, may be obviated, the advantages of this method of observing seem to be sufficient to lead to its adoption in preference to any other yet suggested.

To adapt the cameras for work in this manner, both altitude and azimuth circles were permanently clamped, rendering them immovable in both vertical and horizontal planes, and the locking plates were lifted on the pedestals, so that, while the fiducial lines on the pictures intersect at the zenith, the direction of one of them is that of the line joining the two stations, or the base, the other being at right angles to it.

With the object of ensuring the proper adjustment of the optical axes of the cameras, a tripod stand 12 feet in height was made, which was temporarily erected immediately over them. A plummet was suspended directly above the lens-centre, from the point of intersection of two horizontal wires fixed at right angles to one another, one

of them being carefully made to coincide in direction with the line joining the two cameras.

The charged dark slides, which are separately numbered, so that the correction for each of them may be ascertained and recorded, are then successively placed in the camera and photographs taken of the cross wires overhead, the pictures of which should coincide with the fiducial lines of the camera, the position of which is as nearly as possible adjusted to secure this coincidence. The photographs thus made are preserved, to supply data for correcting the negatives for any error of the fiducial lines, should the slides not be properly adjusted so as to secure the coincidence before spoken of.

Assuming, as may be done without objection for this purpose, that the cloud surface photographed and the earth's surface at the place of observation are in parallel planes, distances measured on the photographs from the intersection of the fiducial lines will represent tangents of angles measured from the zenith to radius equal to the height of the cloud.

Again, if a pair of photographs made simultaneously at the extremities of the base are superimposed one on the other, so that the forms of the clouds coincide, which they will do accurately if the pictures are properly placed, then the line joining the intersections of the cross lines will represent, both in magnitude and direction, the line joining the zeniths of the two ends of the base, from which the observations are made, or the base itself.

If the adjustments before described have been satisfactorily made, the base, as thus indicated, should obviously fall on one pair of the fiducial lines, which, when the photographs are superimposed, should also coincide; otherwise, if the fiducial lines in the two pictures are made to coincide, then the separation of points properly identified in the pictures will be the measure of the parallax or angle subtended by the base at such points.

A scale of angular distance having been prepared as before explained, the parallax thus measured may at once be converted into angular measure, and the height of the cloud is given by the equation

$$H = \beta / \tan \pi,$$

where π is the angular parallax.

In like manner, if two photographs taken from the same point with an interval of time between them be superimposed, so that the cloud pictures coincide, the line joining the intersections of the cross lines will represent in magnitude and direction the movement or drift of the cloud, and the velocity in miles per hour will be found from the equation

$$V = \frac{\delta}{p} \times \frac{\beta}{5280} \times \frac{3600}{t''},$$

where δ and p are the drift and parallax as measured on the photographs, and t the interval in seconds between the pictures being taken.

The method of reduction of the photographs first adopted and employed during the early part of the past summer was as follows:—Prints were made on albuminised paper of the set of four pictures, two taken at each end of the base with an interval of time between them, and they were mounted on stout cards in order to avoid the usual curling up of the paper. When necessary, new fiducial lines were then drawn in the proper direction through the points that had been ascertained to represent the corrected position of the lines of reference as before described, and these lines were extended to the margins of the cards.

If possible, five or six cloud-points were then selected in each print, capable of satisfactory identification. A sheet of paper was next procured, larger than the pictures, and lines intersecting at right angles were drawn across it. Punctures were then made, by means of a needle, through all the selected cloud-points in the four pictures, which were successively placed over the reference sheet (termed hereafter the receiver), so that the fiducial lines upon the pictures coincided with the lines drawn upon the receiver, thereby ensuring the points of intersection being directly superimposed, and, by means of a needle passed through the pricked holes, the marked cloud-points were transferred to the receiver.

This having been done in turn for all the four pictures of the set, the points thus pricked off were joined by inked lines, those obtained from the pair of pictures taken simultaneously being drawn in black ink, and those from the other pair in red, by which a series of parallelograms was formed, equal in number to the number of points selected for treatment.

The black lines or sides of these parallelograms then represented the parallax of the several cloud-points, being proportional in length to the tangent of the angle subtended by the base line at the altitude of the cloud, whilst the red lines forming the other two sides of the quadrilaterals represented on the same scale the drift of the cloud during the interval which elapsed between the taking of the two sets of pictures.

The measurement of these black and red lines provided the means already explained of determining the height of the clouds and the rate of their motion, the direction being given by the inclination of the two lines, of which the black one represented the base.

In dealing with the direction of the drift when thus obtained from positive prints, it has to be remembered that by the printing the right and left of the pictures are transposed, so that the east is on the left and the west on the right in a picture the top of which is directed to the north.

The necessary measurements were made on a scale of millimeters, and the computations carried out by the help of logarithms.

The operations thus described have lately been much abbreviated in various ways. First, it has been found possible to carry out the superposition of the pictures by means of the negatives only, and to work without either employing positives or depending on the identification of a few selected points whose positions were transferred to a receiver.

A frame has been constructed which carries the glass negative plates upon sliders in grooves running in parallel planes, one immediately over the other, but arranged so as to travel at right angles to one another, the lower moving towards and away from the observer, whilst the upper traverses from right to left. A mirror, either a silvered or an opal plate, is employed to reflect the light of the sky upwards to the eye through the negative photograph when the apparatus is placed upon a table in front of a well-lighted window. Stray or diffused light is excluded by placing a box, darkened on its inner surface, over the negatives, and the observer views the combination through a tube fixed perpendicularly upon the top of the box. The two photographs to be compared are placed one in each of the sliding frames, which are first so adjusted that the fiducial lines which follow the direction of the base pass exactly over one another. Next, the bottom or backwards-and-forwards slider is moved until the cloud pictures, say a pair marked A and B, are seen to coincide, and the distance between the intersections of the cross lines on the two plates representing the zenith points, which is the parallax, is then measured by means of a pair of compasses; but a scale could readily be fixed on the slides from which the parallax could be read off without measurement.

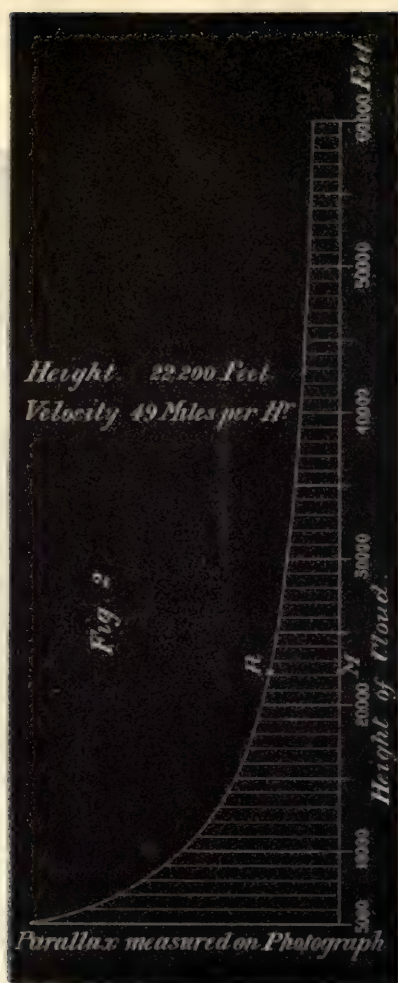
In order to avoid calculations, a standard curve has been drawn (see fig. 2), from which the height of the cloud may at once be graphically determined from the distance between the intersections of the cross lines or parallax of the base as thus measured.

On the axis of abscissæ of this curve are marked off the heights on a scale which makes 2400 feet, the length of the base, equal to the focal distance of the camera, and at regular intervals along this line ordinates are drawn of the length, as measured on the photographs, of the parallax corresponding to the several heights. Through the extremities of these ordinates a curved line is drawn, which gives the locus of the equation

$$h = p \cot \pi,$$

the lengths h and p being both expressed on the scale just mentioned.

The same operations are next performed with pictures A₂ and B₂,



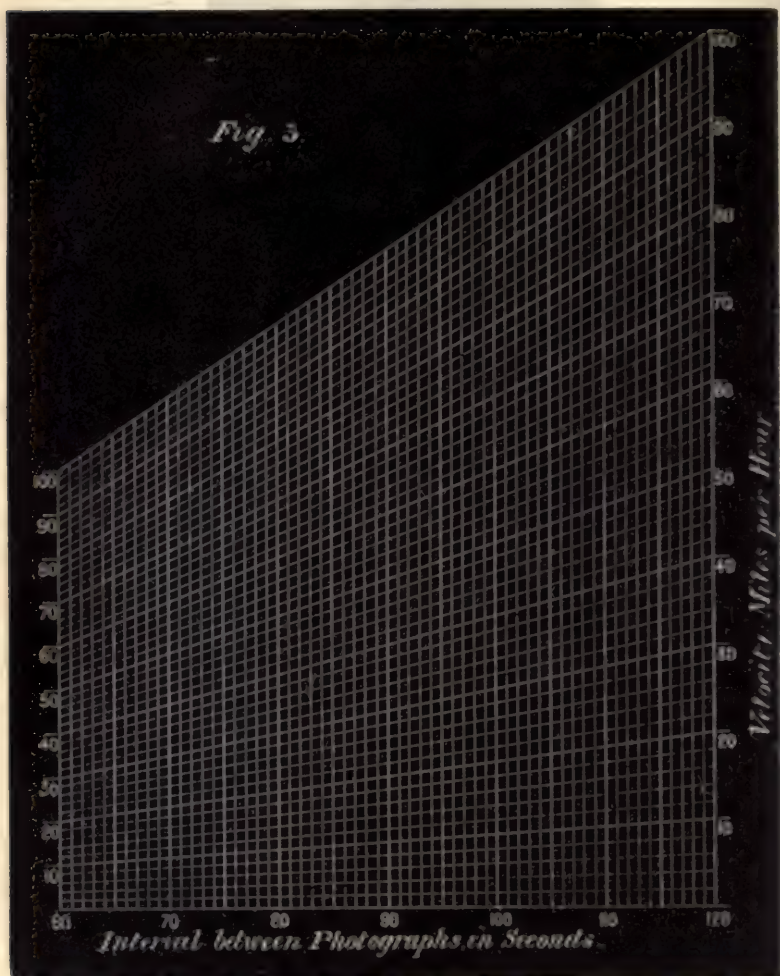
and a second value of the cloud height is obtained, which serves to confirm or modify the first determination.

Then pictures A_1 and A_2 are placed in the frame, and the images superimposed and made to coincide as before, but now the distance separating the zenith of the two pictures, which will be termed the drift, will indicate the space the cloud has moved during the interval between the taking of the two pictures; and the angle which the line joining the zeniths makes with the line of base gives the direction in which the drift has taken place.

From the length of the drift measured upon the plates as above, the velocity of motion may easily be obtained by a graphical method.

As before stated, the velocity in miles per hour is

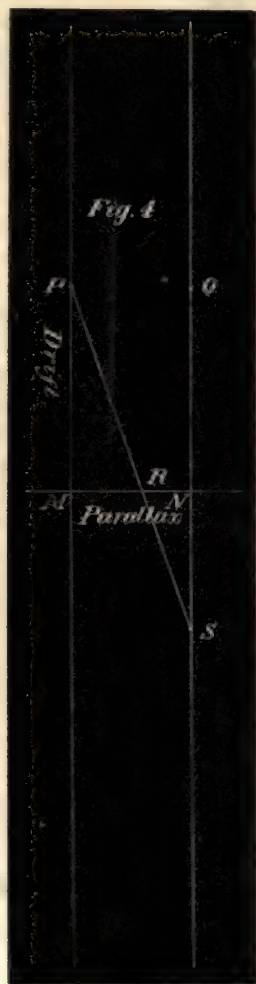
$$V = \frac{\delta}{p} \cdot \frac{\beta}{5280} \cdot \frac{3600}{t''}$$



To obtain the value of V graphically, proceed as follows:—

Draw a horizontal line on which will be represented equal time-intervals from 0 to 120 seconds, see fig. 3. Erect vertical lines at all the points between 60 and 120 seconds, which will include all the time intervals between the pictures likely to occur in practice. On the first of these verticals mark off any convenient length to represent

1 mile, and divide it into 60 equal parts, and from the zero point on the horizontal line draw radiating lines through the points of division, extending to the vertical at 120 seconds. This constitutes a scale of proportional velocities from 0 to 60 miles per hour, and may be extended to any higher velocity. Next (see fig. 4) draw two parallel



vertical lines at a distance apart equal to the length of the base, 2400 feet, on the scale before assumed to represent 1 mile, and draw a horizontal line intersecting the other two at right angles at points M and N.

Then mark off the length of drift δ upwards on each of the two

vertical lines from M and N at points P and Q; and the length of the parallax p , on the horizontal line from M towards N, at a point R. Join P, R, intersecting the vertical through N at S. Then QS represents the drift on the scale assumed to represent 1 mile. Let this be marked off upwards on the vertical line drawn on the scale of proportional velocities, fig. 3, from the seconds division corresponding to the time interval between the pictures, and the velocity of drift will be indicated by the radiating line nearest to the mark thus made.

The scales above described for the graphical determination of the cloud heights and velocities are engraved and printed on sheets of paper, which, after the computations are completed by their aid, will serve as convenient records of the observations.

After a little practice, the whole of the processes requisite for these determinations from the glass plate-negatives of a complete set of four pictures will not exceed 20 minutes. Quite sufficient accuracy is ensured, and the labour and risk of error arising from the use of tables is entirely avoided.

Although the cameras now in use only embrace a circle of angular diameter of about 30° , trials have been made with a lens which gives satisfactory pictures of double that extent, which is probably as much as could be desired.

The following is a list of the determinations made during the past year by the methods now described :—

Date.	Height.	Velocity.	Direction.	Surface.	
				Velocity.	Direction.
1890.	miles.	miles.		miles.	
July 10.....	1·29	7·27	N.W.	10	N.W.
„ 16.....	5·20	45·80	S.W.	5	S.W.
„ 16.....	5·47	41·39	S.W.	5	S.W.
	8·39	64·61	S.W.	5	S.W.
	6·34	49·16	S.W.	5	S.W.
	2·87	15·19	S.S.E.	15	S.W.
August 26.....	1·64	20·19	S.S.E.	15	S.W.
„ 26.....	1·97	13·70	W.S.W.	7	N.
„ 29.....	1·93	13·28	W.S.W.	7	N.
September 9.....	6·87	42·40	W.	3	W.S.W.
„ 9.....	6·29	45·18	W.	3	W.S.W.
„ 10.....	7·22	42·00	N.	8	W.N.W.
„ 17.....	2·60	25·90	S.S.W.	10	S.E.
„ 17.....	2·66	19·90	S.S.W.	10	S.E.
	2·87	19·70	S.S.W.	10	S.E.
	2·27	22·00	S.S.W.	10	S.E.
	4·60	54·40	S.W.	16	S.
„ 18.....	4·60	53·10	S.W.	16	S.
„ 23.....	1·72	5·30	S.W.	5	S.
„ 23.....	1·71	6·40	S.W.	5	S.

II. "The Passive State of Iron and Steel. Part III." By THOS. ANDREWS, F.R.S.S.L. and E., M.Inst.C.E. Received April 23, 1891.

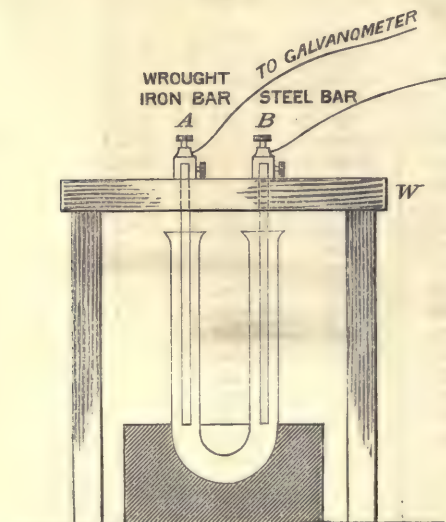
SERIES V, SET 1.

Relative Passivity of Wrought-iron and various Steel Bars, and the Influence of Chemical Composition and Physical Structure on their Passive State in Cold Nitric Acid.

The author is not aware that any previous experiments have hitherto been made showing the relative passivity of the various kinds of steel compared with wrought iron, or the influence of the chemical composition and physical structure of such metals on their passive condition in nitric acid.

The passive state of iron or steel may have hitherto been regarded by many as a sort of fixed property pertaining to iron and steel alike, when immersed in cold, strong nitric acid. The following experiments were made to investigate if the passivity was of an universally static character, or whether it varied with the chemical composition and general physical structure of the metal and, if so, to what extent. For convenience, this part of the investigation was divided into two parts, one portion of the observations, Set 1, being made on drawn rods of metals of known chemical composition and structure, and the other, Set 2, of experiments constituting a study of the relative

FIG. 5.



passivity of various steel and iron plates of known but varied composition, &c. The experiments of Set 1 were made on bars of the various steels selected from the author's standard samples. The

Table VI.

Time from commencement of experiment.	Current between polished "passive" wrought-iron and steel bars in cold nitric acid 1.42 sp. gr. Electro-chemical position of the wrought iron positive, except where otherwise marked N (negative). E.M.F. in volt.					
	Column 1.		Column 2.	Column 3.		Column 4.
	Soft cast steel with wrought iron.		Hard cast steel with wrought iron.	Soft Bessemer steel with wrought iron.		Tungsten steel with wrought iron.
	Set No. 1.	Set No. 2.	Set No. 3.	Set No. 4.	Set No. 5.	Set No. 6.
seconds						
0	0.000					
30	0.013	0.022 N	0.004 N	0.017	0.016	0.070 N
minutes						
1	0.005	0.022 N	0.016 N	0.022	0.017	0.074 N
3	0.005 N	0.022 N	0.020 N	0.030	0.024	0.073 N
5	0.007 N	0.028 N	0.023 N	0.034	0.032	0.071 N
10	0.011 N	0.026 N	0.022 N	0.034	0.034	0.070 N
20	0.012 N	0.025 N	0.020 N	0.031	0.034	0.065 N
30	0.013 N	0.023 N	0.023 N	0.028	0.032	0.061 N
40	0.013 N	0.019 N	0.020 N	0.024	0.029	0.060 N
50	0.013 N	0.017 N	0.019 N	0.023	0.026	0.059 N
hours						
1	0.013 N	0.014 N	0.019 N	0.020	0.024	0.056 N
1½	0.012 N	0.011 N	0.020 N	0.017	0.019	0.055 N
2	0.011 N	0.008 N	0.020 N	0.014	0.016	0.054 N
2½	0.007 N	0.005 N	0.019 N	0.012	0.013	0.052 N
3	0.004 N	0.001 N	0.018 N	0.012	0.013	0.052 N
3½	0.002 N	0.000	0.018 N	0.011	0.013	0.051 N
3¾	0.000	0.001	0.017 N	0.011	0.013	0.050 N
4	0.002	0.004	0.016 N	0.011	0.012	0.049 N
5	0.006	0.007	0.013 N	0.011	0.011	0.049 N
7	0.016	0.012	0.006 N	0.011	0.011	0.048 N
12	0.037	0.018	0.006	0.012	0.011	0.048 N
18	0.052	0.026	0.017	0.013	0.012	0.047 N
20	0.058	0.030	0.023	0.013	0.013	0.047 N
22	0.064	0.033	0.028	0.014	0.015	0.048 N
24	0.070	0.036	0.033		0.016	0.065 N
26	0.078		0.035			
29	0.085		0.042			
30	0.088		0.047			
38	0.098		0.058			
40	0.107		0.060			
43			0.065			
45			0.071			
47			0.090			

bars were cold drawn through a wortle, and were therefore different in physical structure to the rolled plates used in the second series of the experiments. An idea of their general properties will be obtained on reference to Part II, Tables IV and V. A polished bar, $8\frac{1}{4}$ inches long, 0.310 inch diameter, of the steel to be tested was placed in the wooden stand W (fig. 5), along with a polished wrought-iron bar of equal size, and the pair were then immersed in $1\frac{1}{4}$ fluid ounce of nitric acid 1.42 sp. gr., contained in the U-tube, the bars being in circuit with the galvanometer. The immersion was continued for the periods stated, and with the electro-chemical results given on Table VI.

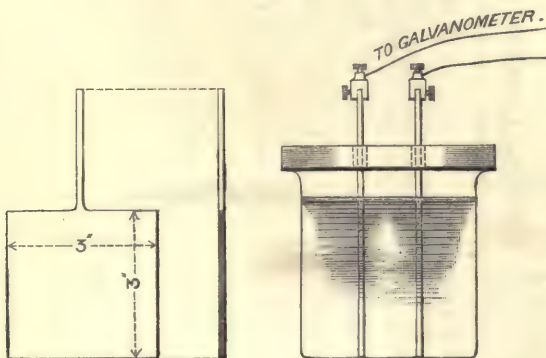
The wrought-iron bars used in each experiment were cut from one longer polished rod, so as to afford a fair comparison of the relative passivity of the various steels, compared with the wrought iron and also with each other. The results are the average of numerous experiments in each case.

SERIES V, SET 2.

Relative Passivity of Wrought-iron and various Steel Plates in Cold Nitric Acid sp. gr. 1.42.

In the following series of observations, the metals experimented upon consisted of plates of rolled wrought iron, rolled steels made by the Bessemer, Siemens-Martin, or crucible cast-steel processes, and they were of the chemical composition given on Table VII. Each plate was 3 inches square, by $\frac{1}{8}$ inch thick, = total area of exposure, 19.5 square inches including edges, brightly polished all over, and had a long thin strip left on the top side (see fig. 6), for convenience

FIG. 6.



of attaching to the galvanometer connexions. The whole of the wrought-iron plates, used as elements with the various steel plates,

Table VII.
Chemical Analysis of the Wrought-iron and Steel Plates used in the Experiments.

Description.	Combined carbon.	Silicon.	Sulphur.	Phosphorus.	Manganese.	Iron (by difference).	Total.
Wrought iron (Wortley best scrap) ...	per cent. none	per cent. 0·392	per cent. 0·034	per cent. 0·270	per cent. 0·194	per cent. 99·110	per cent. 100·000
Soft cast steel	0·460	0·074	0·025	0·210	0·184	99·047	100·000
Hard cast steel	1·407*	0·121	0·056	0·080	0·360	97·976	100·000
Soft Bessemer steel	0·150	0·015	0·111	0·064	0·540	99·120	100·000
Hard Bessemer steel	0·510	0·068	0·113	0·087	1·153	98·069	100·000
Soft Siemens steel	0·170	0·071	0·117	0·077	0·627	98·938	100·000
Hard Siemens steel ...	0·720	0·080	0·102	0·143	1·239	97·716	100·000

* By combustion. The terms "soft" and "hard" relate only to difference of percentage of combined carbon, and not to their having undergone annealing or hardening processes.

Table VIII.

Time from commencement of experiment.	Current between bright "passive" wrought-iron and steel plates in cold nitric acid 1.42 sp. gr. Electro-chemical position of the wrought iron positive, except where otherwise marked N (negative). E.M.F. in volt.					
	Soft cast steel with wrought iron.	Hard cast steel with wrought iron.	Soft Bessemer steel with wrought iron.	Hard Bessemer steel with wrought iron.	Soft Siemens steel with wrought iron.	Hard Siemens steel with wrought iron.
minutes.						
1	0.020	0.071	0.017 N	0.076	0.031	0.065
5	0.032	0.074	0.005	0.079	0.017	0.064
15	0.038	0.073	0.013	0.086	0.024	0.061
30	0.040	0.067	0.012	0.098	0.038	0.064
40	0.048	0.062	0.012	0.107	0.048	0.064
50	0.049	0.059	0.011	0.104	0.053	0.064
hours.						
1	0.047	0.055	0.011	0.103	0.053	0.064
2	0.047	0.061	0.007	0.109	0.034	0.062
3	0.048	0.060	0.000	0.103	0.013	0.061
4	0.047	0.060	0.013	0.098	0.065	0.066
5	0.048	0.058	0.019	0.121	0.007 N	0.060
6	0.050	0.052	0.007 N	0.106	0.022 N	0.056
8	0.038	0.053	0.011 N	0.104	0.037 N	0.059
9	0.040	0.054	0.011 N	0.107	0.034 N	0.058
15	0.053	0.061	0.024 N	0.086	0.017 N	0.055
18	0.060	0.055	0.030 N	0.077	0.008 N	0.056
20	0.050	0.054	0.038 N	0.077	0.007 N	0.058
22	0.040	0.060	0.028 N	0.079	0.007 N	0.061
24	0.038	0.060	0.023 N	0.077	0.007 N	0.064
26	0.046	0.064	0.017 N	0.065		0.064
28	0.049	0.065	0.013 N	0.061		0.062
30	0.050	0.073	0.017 N	0.061		0.066
32	0.049	0.071	0.028 N	0.061		0.070
40	0.052	0.067	0.016 N	0.064		0.084
45	0.050	0.077	0.015 N	0.070		0.090
50	0.046	0.077	0.018 N	0.070		0.088
54	0.046	0.077	0.017 N	0.071		0.086
56		0.078	0.016 N	0.071		
66		0.078	0.017 N			
72		0.067				

were cut from one larger wrought-iron plate and were thus practically of uniform composition, thus ensuring an accurate comparison of the relative passivity of the wrought iron compared with the different types of steels, and at the same time indicating relatively the influence of varied composition and structure on the passivity of the different classes of steel under observation. In each experiment, a polished wrought-iron plate and a polished steel plate were firmly placed in two small holes drilled through a thick plate-glass cover;

the cover holding the two plates was then carefully placed closely over a porcelain vessel containing 15 fluid ounces of nitric acid sp. gr. 1.42, the plates being fully immersed in the acid, and the protruding shanks of the bars connected in circuit with the galvanometer. The electro-chemical effects observed were then taken in the usual manner, and the results are given on Table VIII.

At the conclusion of each experiment on Table VIII, the nitric acid, though quite colourless at first, was found to be of a yellowish-brown colour. A small deposit of fine black carbonaceous-looking matter was noticed at the bottom of the tank surrounding the wrought-iron bar in each set of these experiments.

The hard Siemens-Martin steel plate and the wrought-iron plate, instantly after withdrawal from the acid, showed nearly their original bright polish, with the exception of a few fine streaks or markings on the wrought-iron plate, indicating that the latter metal had been rather more acted upon than the steel plate, the hard Siemens-Martin steel plate presenting a slightly dull-greyish aspect. Somewhat similar results were observed on withdrawing the soft cast steel, hard cast steel, soft Bessemer steel, and hard Bessemer steel series of plates from the nitric acid.

The hard cast steel plate when taken out showed a dull lustre much removed from its original bright polish, but there were no other signs of its having been acted upon. The wrought-iron plate connected with it was bright on withdrawal from the liquid and but very slightly marked.

General Remarks.

It has been necessary to give in modified detail the effects observed during the periods of experimentation recorded on the Tables, Parts I, II, and III, so as to convey an accurate intimation of the method and nature of the research, and a brief *résumé* of some of the principal results and conclusions arrived at by the author up to the present time may now be given.

Firstly.—The experimental observations of Part I, Series I, indicate that the influence of magnetisation on the passive state of steel rods in cold nitric acid sp. gr. 1.42 is not very great, but it was detectable with the delicate galvanometer and by the sensitive electro-chemical method pursued by the author in the investigation.

The effect of magnetisation is more marked in warm nitric acid, and when the iron is in a powdered state, as shown in the independent and separate experiments of Messrs. Nichols and Franklin on passive powdered iron in warm nitric acid, previously alluded to in Part I, by whom it was shown that the temperature of transition from the passive to the active state was very materially lowered by powerful magnetism; their experiments also indicate that the passive state of

powdered iron cannot be fully overcome, even under strong magnetic influence, until a temperature of about 51° C. is reached.

Secondly.—The author's experiments of Part I, Series II, at higher temperatures confirm those of Part I, and further tend to demonstrate the influence of magnetisation in somewhat lessening the passivity of steel, showing that even previous to the critical temperature point of transition from the passive to the active state, magnetised steel bars were rather less passive in warm nitric acid than unmagnetised ones.

Thirdly.—The results in Part II, Series III, show that the passivity of both unmagnetised wrought iron and unmagnetised steel in nitric acid sp. gr. 1.42 is considerably and proportionately reduced as the temperature of the acid increases, until the temperature point of transition from the passive to the active state is reached at a temperature of about 195° F., and it was also found that the wrought iron was less passive in the warm nitric acid than cast steel; see also remarks at foot of Diagram I, in Part II.

Fourthly.—The results of the observations of Part II, Series IV, indicate that Scheurer-Kestner was to some extent in error in regarding the passivity of iron as not dependent on the greater or less degree of saturation of the acid. The author's experiments herein recorded have shown that the passivity of the metals employed, viz., wrought iron, soft cast steel, hard cast steel, soft Bessemer steel, and tungsten steel, was very materially increased with the concentration of the nitric acid, and it was also observed that wrought iron was much less passive in the nitric acid of less concentration than most of the steels, the soft Bessemer steel being found about equal in passivity to the wrought iron under the conditions of experimentation. A reference to Table III shows that a considerable amount of E.M.F. was developed between the different metals in every instance, which is a circumstance of much interest in connexion with the passive state of iron and steel.

Fifthly.—The results obtained in Part III, Series V and VI, on the relative passivity of wrought iron and the various steels, soft cast steel, hard cast steel, soft Bessemer steel, hard Bessemer steel, soft Siemens steel, and hard Siemens steel, are of an important character, showing, by the delicate electro-chemical method employed, the powerful influence of difference in chemical composition and physical structure, &c., on the passive state of the metals. Generally throughout this series of experiments it will be observed that the wrought iron was electro-positive to the steels with a considerable E.M.F., amounting in some cases to as high as one-tenth to one-seventh of a volt, the wrought iron being thus shown to be less passive than the steels. In the experiments on the wrought-iron and various steel bars on Table VI, which in course of their manufacture were drawn cold through a wortle, and were hence in a different molecular condi-

tion to the plates (which were rolled hot) experimented upon in Table VIII, it will be noticed that, in several instances with soft cast steel and hard cast steel, the wrought iron did not assume the electro-positive position until two or three hours after immersion in the nitric acid. Subsequently the iron assumed its normal position, and became during the long remaining period of the observations electro-positive to the steels, with a considerable and increasing E.M.F., showing that the wrought iron was becoming gradually very much less passive than the steels. In the case of the soft Bessemer and soft Siemens plates, Table VIII, we have also a similar instance of these peculiar and temporary interchanges and variations of relative passivity which are not easily accounted for. In the case of the tungsten steel, Table VI, the wrought iron was steadily in the electro-negative position, hence in the latter instance showing the wrought iron to be permanently more passive than the tungsten steel.

A reference to the experiments on the wrought iron and various steel plates, on Table VIII, shows that the E.M.F. between the passive wrought iron and the various soft steels, which contained less percentage of combined carbon, in circuit in cold nitric acid sp. gr. 1.42, was very considerably less than the E.M.F. under similar conditions between the wrought-iron plates and the different hard steels having a higher percentage of combined carbon. The latter results, therefore, demonstrate the interesting circumstance that steels, of a higher percentage of combined carbon are more passive than those of a lower percentage of combined carbon. It will be observed that the wrought iron was also electro-positive to most of the steels, whether of a higher or lower percentage of combined carbon, which shows that wrought iron may be regarded as generally less passive than steels.

III. "On the Demonstration of the Presence of Iron in Chromatin by Micro-chemical Methods." By A. B. MACALLUM, M.B., Ph.D. Communicated by Professor H. N. MARTIN, F.R.S. Received April 23, 1891.

(Abstract.)

The method of isolating what is called chromatin by the histologist yields compounds of fairly stable composition called nucleins, some of which have been shown to contain iron (Bunge and Zaleski). My observations on hæmatopoiesis in Amphibia led me to the conclusion that the chromatin, from which the hæmoglobin of the hæmatoblasts is derived, is an iron-holding compound. Other observations indicated that the conclusion could, possibly, be made of general application, *i.e.*, that iron is present in the chromatin of every cell, animal and

vegetable. The ordinary method of isolating chromatin employed in chemical and physiological laboratories cannot be readily applied in testing the correctness of this supposition. It is conceivable that this substance absorbs and retains tenaciously iron-holding compounds as readily as it does some of the dyes used by the histologist. It is not easy to remove such compounds without, possibly, decomposing the chromatin, and, when the latter is prepared in any quantity, one cannot be certain that the iron which is present may not be an impurity. To overcome this difficulty, one must prepare chromatin from organs which are free from hæmatin or like substances, or from inorganic iron compounds, and, for this purpose, fairly large quantities would be necessary for chemical manipulation. There is, apparently, no organ, animal or vegetable, which offers such an opportunity. There consequently remains but one other way by which the view, that iron is constantly present in chromatin, can be put to the proof, and that is the micro-chemical one. I have found that a certain method of employing ammonium sulphide as a reagent for iron shows the presence of the latter in the chromatin of the nuclei of a very large number of species of cells hardened in alcohol. The iron in this case does not occur combined as an albuminate, but rather in a condition which, as regards the firmness of the combination, is comparable to that present in potassium ferrocyanide or hæmatin. That the iron found is not due to the presence of hæmatin is shown by the results of experiments made with vegetable cells, and with animal cells which one would not naturally expect to contain hæmatin, as, for example, those of the corneal epithelium in Amphibia. In support of this may also be mentioned the fact that where chromatin is very abundant the iron reaction is very marked, while it is feeble in cells poor in chromatin. In the chromatin loops and filaments of karyokinetic figures the iron reaction is intense and sharply confined to these structures.

I forego, for the present, any expression of opinion as to the general application of the results obtained. I would not even maintain that the chromatin of every cell essentially contains iron, although my studies have, so far, not furnished an instance which can support the contrary view.

The Society adjourned over Ascension Day to Thursday, May 14.

Presents, April 30, 1891.

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The Author.

May 14, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes the names of the Candidates recommended for election into the Society were read from the Chair as follows:—

Anderson, William.
Bower, Prof. Frederick Orpen,
D.Sc.
Conroy, Sir John, Bart., M.A.
Cunningham, Prof. Daniel John,
M.D.
Dawson, George Mercer, D.Sc.
Elliott, Edwin Bailey, M.A.
Frankland, Prof. Percy Faraday,
B.Sc.

Gilchrist, Percy C.
Halliburton, William Dobinson,
M.D.
Heaviside, Oliver.
Marr, John Edward, M.A.
Mond, Ludwig.
Shaw, William Napier, M.A.
Thompson, Professor Silvanus
Phillips, D.Sc.
Tizard, Capt. Thomas Henry, R.N.

The following Papers were read:—

- I. "On the Examination for Colour of Cases of Tobacco Scotoma, and of Abnormal Colour Blindness." By Captain W. de W. ABNEY, C.B., R.E., D.C.L., F.R.S. Received April 29, 1891.

The following cases were submitted to the Colour Vision Committee, and it was thought desirable that the results of the examination should be communicated to the Royal Society.

The examination of these three cases was conducted at different times, the first partially in the presence of the Colour Vision Committee by Mr. Nettleship, and the two last, and part of the examination of the first, at different times, in my laboratory, with the assistance of Mr. Nettleship.

In all three cases the examination made was an ability to distinguish colour, luminosity of the different parts of the spectrum, and total sensation of light; and, in addition, in the first case, to range of colour sensation on the retina.

Case I.—This patient, Alfred C., aged 36, a traveller, was suffering from rather severe tobacco amblyopia, and was brought to the Committee by Mr. Nettleship. The scotoma was a very marked one, and the loss of colour sensation most complete. Mr. Nettleship has kindly added the following remarks on the case:—

His acuteness of vision was $\frac{5}{36}$ with R. and $\frac{6}{60}$ with L. He smoked half-an-ounce of “shag” daily and drank about four pints of beer. His sight had been failing for about two months. As is common in early stages of this disease, the ophthalmoscope revealed no decided changes at the optic discs.

He was tested at the Royal Institution by Mr. Nettleship, in the presence of the Committee, with the following results:—

He passed the test of the Holmgren wools satisfactorily, proving that the usual vision was normal for colour. I had prepared small pellets of moulder’s clay, each weighing 4 grains, and about $\frac{1}{8}$ inch in diameter, and had had sets coloured with the same colours as those of the Holmgren wools. C. was told to pick out the blues, reds, and greens. The blue pellets he picked out without fail, and he never made the least mistake in his choice, but he failed entirely to distinguish the greens or reds, mistaking them for drabs and greys, which were amongst the pellets. When told to look away some 20° from the slab on which the pellets were placed, he at once saw all the colours, but directly he turned his eyes to pick them out, all colour perception, except for blue, disappeared. This test indicated that he had lost all perception of green and red in the central part of the eye. He was next tested with small discs of different colours by Mr. Nettleship, keeping his eye fixed on a given point, and the loss of colour sensation for all except blue, and perhaps a little yellow, in the central part of the eye, was at once made apparent; the blue he would distinguish with the greatest facility, and the sensation was apparently as strong as in normal eyesight. A further test was made by Mr. Nettleship with coloured lights to imitate signal lights, and he named a brilliant red light, and an equally brilliant green light, when side by side, both as white (see also p. 85).

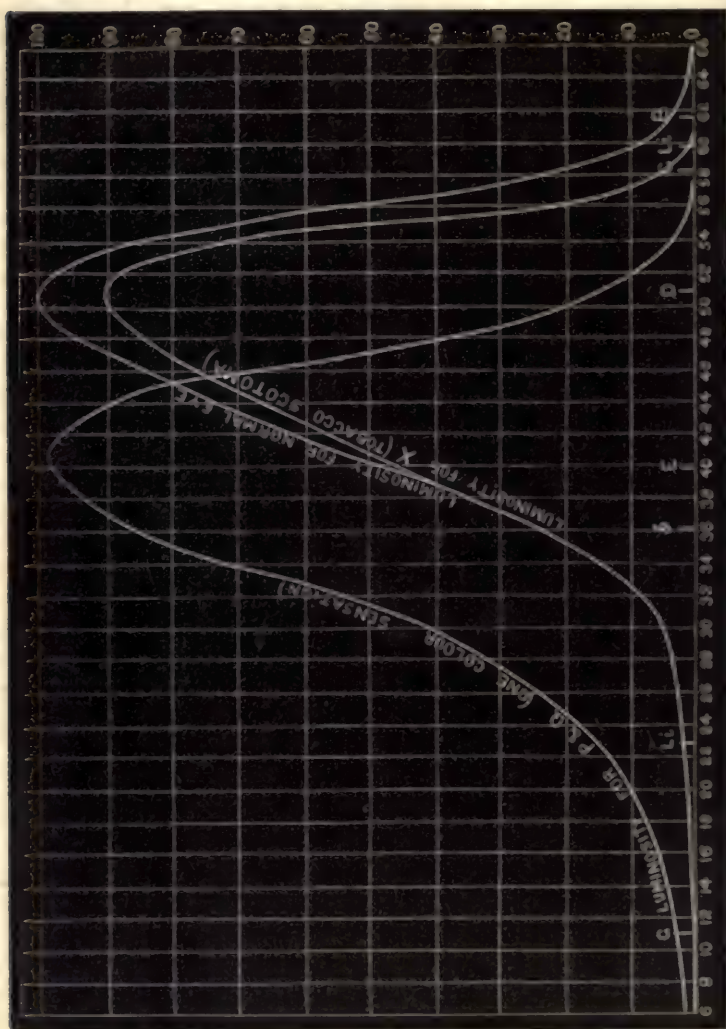
This man attended at my laboratory, at the meeting of the Committee on Colour Vision, with Mr. Nettleship, and he was tested with

the colour-patch apparatus described in "Colour Photometry," in the 'Philosophical Transactions,' 1886, by General Festing and myself. The objects first in view were to test his perception of the spectrum colours, and then his retinal field colour perception for the same. A template was cut out after the manner described by General Festing and myself in the second part of "Colour Photometry" ('Phil. Trans.,' 1889), of such a shape that all the spectrum lying between $\lambda 4600$ and $\lambda 6600$ was reduced to equal luminosity when it was rotated in front of the spectrum. Diaphragms containing holes of different sizes were placed in front of the last prism, and thus a round spot of monochromatic light of the same luminosity was produced upon the screen when a slit was passed through the spectrum. From the red end to $\lambda 5270$ he called the whole of the colours white, and from that point he began to see blue, called the colours bluish and blue. When the full illumination for all the colours was used, the same results were obtained. From this examination it would appear that he was totally deprived of the sensation of any colour except of blue. A subsequent examination of his perception of the luminosity of different rays, however, has to be taken into account, for in the first examination he had no light of pure white with which to compare the colours. In the next experiments, a strip of white light was

I.	II.	III.	IV.	V.	Remarks.
Scale No.	Wave-length.	Luminosity to the normal eye.	Luminosity to X.	$\frac{IV.}{III.}$	
60	6730	7.3	0	0	Sees only the white stripe.
57	6423	32	10	0.31	Calls red yellowish, and white bluish.
55	6242	65	38	0.65	" " "
53	6074	96	86	0.89	Both one colour.
51	5920	99	90	0.91	" "
47	5660	92	83	0.90	Calls green a little blue; white he sees as white.
43	5430	69	625	0.90	" " "
40	5270	50	46	0.92	" " "
32	4910	8.5	9	1.06	Sees blue as blue, and white yellowish.
31	4960	7	8	1.14	" " "
26	4680	3	3	1.00	" " "

placed in juxtaposition to the colour, and the results were slightly different. The table (p. 493) gives his luminosity measures. Col. I is the empiric scale number, II is the wave-length, III the luminosity of the colour to the normal eye, IV the luminosity to C, and V the ratios of III to IV.

FIG. 1.



In the diagram, his luminosity curve X is shown, its area being 1400 against 1650 for the normal eye. As will be shown, his perception of light is only two-thirds of that of the normal eye; hence

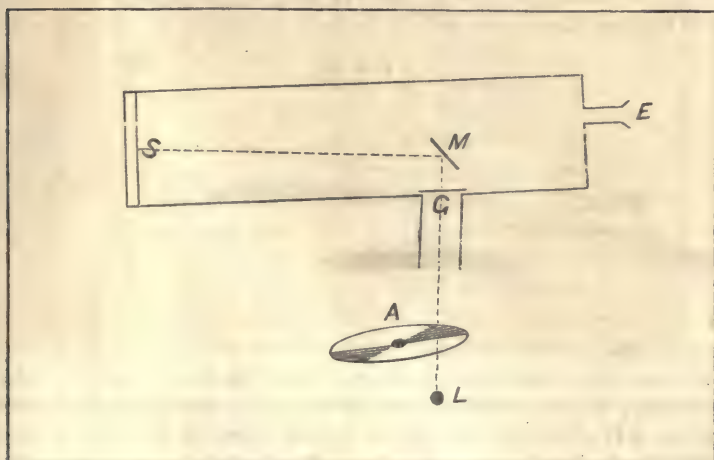
his area of luminosity should be 1100. As it is 1400, the ordinates of the above curve should be multiplied by 0·8, to compare with that of the normal eye.

It should be mentioned that his matches of luminosity were made without any hesitation, and were concordant for each observation, which is not to be wondered at, as the matches, except at the blue end, were practically matching shades of black to white.

From the foregoing, it will be seen that the white which C. sees as white is the same as the D sodium light, and that the red he says is yellowish. The mixture of this yellowish-white with the blue apparent makes white at λ 5430. He sees a little blue in the spectrum colour at λ 5720, so it must be taken that at that point of the spectrum he begins to see colour, a point which is considerably lower than that given by his preliminary examination of the spectrum colour, and due, no doubt, to the fact that the white light used by the comparison light was that of the positive pole of the electric light. It seems probable that what C. called yellowish was really a sensation of white mixed with a very small quantity of red sensation (as he saw no yellow in the orange, in which that colour would be most easily distinguished on account of its luminosity), and red light, when strongly diluted with white light, to the normal eye appears slightly orange.

Subsequently C. was tested for the illuminating value of white light compared with my own and that of Mr. Nettleship. The apparatus used in this case (fig. 2) was, I believe, somewhat on the principle of Dr. Förster's photometer, with which I was unacquainted before I made the instrument. It is made as follows:—

FIG. 2.



E is a small tube for the eye to look down into a box 4 feet long; G is an aperture in the side of the box covered with ground glass; L is a gas-light; A rotating sectors which can be opened and closed at will; M a mirror to reflect the light on to a card (which can be changed at will, and on which are one or more black spots) slipping into a slot S from the top of the box. E is so arranged that the whole of the card can be viewed. The observer places his eye at E, and the sectors, which at first are closed, are gradually opened until the observer can see that there are black spots on a white ground. The angle of the aperture of the sector is noted. Each eye is tested.

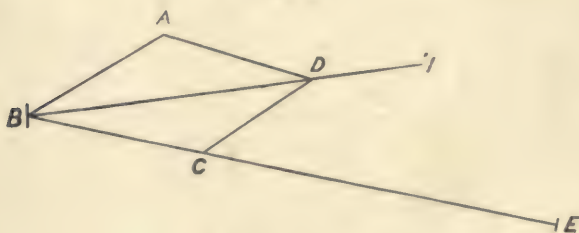
In this case my own right eye, agreeing with that of Mr. Nettle-ship, was used as the standard, since it was with that that the normal luminosity curve was originally made.

	Right eye.	Left eye.	Remarks.
Abney.....	17	17	5 smallish spots used as test object.
Alfred C. ..	31	25	" " " "
Abney.....	10	9	1 large spot " "
Alfred C. ..	26	17	" " " "

From this it may be concluded that C.'s appreciation of light to the standard is about half for the right eye, and two-thirds for the left that of the standard.

The horizontal colour field was tested by a modification of the colour patch apparatus.

FIG. 3.



A brass-work frame was made as shown, fig. 3. A and B are fixed to a board, the other arms are capable of moving with parallel motion, the arm BD slides through D; at B, and attached to BD, is a mirror which can be fixed in any position. The light, when once turned in

the direction BL, always falls on E, at the end of which a paper disc can be placed. A mirror without this arrangement can be employed, the light falling on a paper strip, but is not quite so convenient. The monochromatic light was thrown on the mirror, and the angular deviation from the zero point read, the eye being fixed on a point along the zero reading. The experiments were made first with the ordinary luminosity of colours, and subsequently with the reduced.

Scale No.	Wave length.	Temp. side.	Nasal side.	
56	6330	14°	10°	} Square patch, 2-inch side.
52	5996	14	10	
47	5658	15	11	
56	5330	14	10	
52	5996	14	10	} Small round spot, ¼-inch diam.
47	5658	white all over the field		
23	4600	sees blue throughout.		

In the above, the angles show where colour was first visible.

Case II.—The next two subjects are brothers (Alfred and William P., indicated below as P and Q), whose colour perception is monochromatic. Mr. Nettleship had previously tested them with wools, and the matches they made, such as matching yellow with blue, made it evident that their colour vision was very abnormal. The defect is not due to active disease, but they were born with it. They suffer from amblyopia. These cases were published as cases of "Day-blindness with Colour-blindness," by Mr. Nettleship, in the 'St. Thomas's Hospital Reports,' 1880 (vol. 10).

Testing them with the spectrum, they made most extraordinary mistakes, calling blue, red; red, green or blue. On cross-examination, it seems that they only distinguish colour by its luminosity; they always explain that one colour is lighter or darker than another; evidently their colour names are founded on the observation of what is told them as to the different colours, and not from any real knowledge of them. The next examination was to get their luminosity curve of the electric light spectrum, and this they did with the very greatest ease. Their readings for the same colour were occasionally a little erratic, differing as much as 5 per cent. from one another, but, by taking the means, their curves come out very concordantly. Practically, the curves of the two brothers are identical, the means not differing 2 per cent. from one another at any part of the spectrum; hence it is unnecessary to give more than one of them. It should be mentioned that both brothers could just catch a glimpse of the red line of lithium when the spectrum of the vapour was on the focussing screen of the apparatus.

The luminosity curves are shown in the diagram, and the following is the table of observations:—

I.	II.	III.	IV.	V.	Remarks.
Scale No.	Wave-length.	Luminosity to the normal eye.	Luminosity to P and Q.	$\frac{IV.}{III.}$	
55	6242	66	1.2	0.02	"Both blue."
54	6156	84	2.4	0.03	
52	5996	97	9.6	0.10	
50.6	5990	99	14.4	0.145	(D line).
48	5720	95	33.6	0.35	"Both white."
46	5596	86	62.5	0.73	
44	5480	75	86.4	1.15	
42	5370	62.5	92.6	1.49	
40	5270	50	96	1.92	"Both white." (39.8, E line.)
38	5180	36	93.6	2.63	" (38, "b" line.)
36	5080	24.5	86.4	3.36	
34	4990	15.0	77.4	5.16	
32	4910	8.5	60.0	7.06	
30	4820	5.5	41.0	7.45	"Both blue."
25	4650	2.5	21.6	8.64	
20	4510	1.4	9.6	6.85	
10	4270	0.6	2.4	4.00	

Their appreciation of light was—

	Right eye.	Left eye.
Abney	23	25
P	23	16
Q	15	15

We may take it that P's right eye has the same appreciation of light as the standard, and his left 1.5 times that of the standard. Q is 1.6 times that of the standard with both eyes. The luminosity curves were taken with both eyes open.

The curve is very remarkable, showing an intense excitement by the blue rays of the spectrum, the whole of which appears of one colour to both brothers. The maximum luminosity is about E, but

the place of the greatest difference in degree of luminosity, compared with the normal, is near F, where it is more than eight times more luminous than the normal eye. The area of their luminosity curve is 1800; that of the normal eye being 1650. If we take their appreciation of light as being 1.5 that of the standard, their luminosity curve should be 1650 by 1.5 or 2475. Theirs is 1800 as measured, the ordinates of their curve should therefore be multiplicatively 1.37 for a strict comparison with that of the normal eye. We may therefore take it that near F their sensation of light is 11.83 times that of the normal eye.

In the three patients we have cases of abnormal vision, one in which practically the only sensations in the central part of the eye are white and blue, and in the other two there is only one sensation. In these last two cases we have apparently a curve of the fundamental sensation, since it must be the same as the luminosity curve, and it appears to agree with that found by Koenig. In regard to Alfred C., it should be remarked that he begins to feel the blue sensation in the spectrum near the point where Koenig places its origin.

Addendum. Received June 18, 1891.

On Four Cases of Colour Blindness Examined for the Colour Vision Committee.

Case III.—The next case, W. S., is one of progressive atrophy of both eyes. When tested with spectrum colours—a patch of white light being placed in juxtaposition with the colour—it was found that he was absolutely blind to colour from 26.75 (λ 4733) on the scale of the spectrum to the termination of the red of his spectrum, which was close to 63 on the scale (λ 7082). Above scale No. 26.75 W. S. saw blue, and his spectrum was continued normally in the violet. Mr. Nettleship has promised to furnish a chart of his retina. His luminosity curve (fig. 4) was made without any difficulty, and, compared with my own, is slightly deficient, from the red to the yellow, but his perception of luminosity increases as the blue is approached.

The following is the table applying to his curve of luminosity :—

Scale No.	Wave-length.	Reading.	Remarks by W. S.
60	6728	3.4	Grey.
58	6520	15.0	"
56	6330	41.0	"
55	6242	43	"
54	6152	69	
52	5996	94	
50	5850	100	"
48	5720	96	
45	5538	88	
42	5373	74	
40	5270	61.5	"
38	5172	45	
35	5042	30	
30	4848	12	"
25	4675	6	Bluish.
20	4518	4	
15	4376	3	Blue.
10	4248	2.5	"

He was subsequently tested with colour discs—Ultramarine (U), Red-royal (R), Emerald-green (G), Chrome-yellow (Y), White (W), and Black (B).

It was found that—

$$165 (U) + 48 (R) + 147 (G) = 75 (W) + 285 (B).$$

The black reflected 3.4 of white; hence the true equation is—

$$(i). 165 (U) + 48 (R) + 147 (G) = 84.7 (W) + 275 (B).$$

$$(ii). 120 (U) + 240 (Y) = 196 (W) + 164 (B) \text{ (corrected)—}$$

With 260 (U) + 100 (Y) he sees blue.

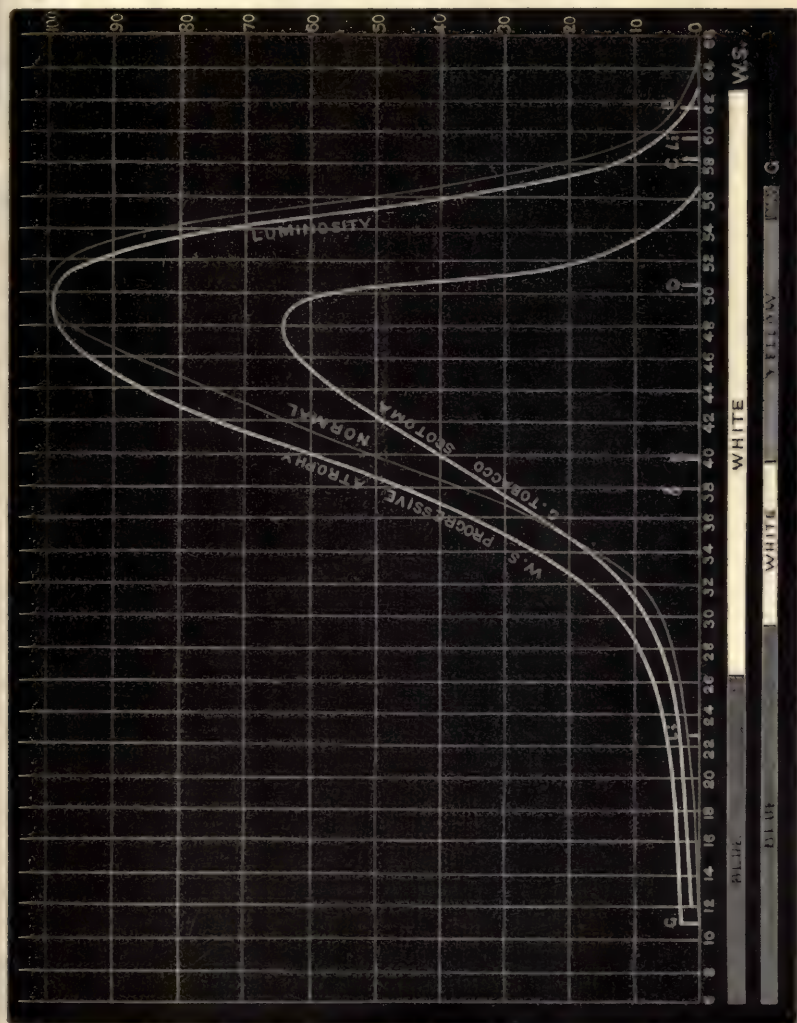
250 (U) + 110 (Y) „ light-blue.

242 (U) + 118 (Y) „ no blue.

This last in connexion with (ii) shows that his blue perception is

neutralised by the yellow, although the yellow to him was matched with white.

FIG 4.



The thin line curve is the normal curve.

Case IV.—The next case is that of G., suffering from very well marked tobacco scotoma, occupying a considerable area. His curve of luminosity of the spectrum is shown in fig. 4. The following table refers to it:—

Scale No.	Wave-length.	Reading.	Remarks by G.
57	6423	0	
55	6242	3	No colour.
53	6074	11	Colour "yellow," white "blue."
51	5919	34	" " " "
50	5850	60	" " " "
49	5783	64	Colour "gold," white "sky-blue."
45	5538	59	
40	5270	40	Both white.
35	5042	18	"
30	4848	10	"
29	4807	6	Colour "very pale blue," white as white.
26	4707	4	Colour "blue," white "white."
20	4518	3	" " " "
10	4248	2	" " " "

G. was tested for light sense in the apparatus described in the previous memorandum.

Light disappeared with

	Right eye.	Left eye.
Abney	5°	5°
Nettleship	5°	5°
G.	55.58.58	58.58

It thus appears that the final sensitiveness to light of the central part of the eye was nearly 12 times less than a person possessing normal sense.

Case V.—This was a remarkable case, which Mr. Nettleship had mentioned to the Committee. He had stated that this lady, N. W., mistook blue for red, and it was with some curiosity that this case was examined. Her first examination was as to colour sense with the spectrum colours, a patch of mono-chromatic light being placed in juxtaposition with an equal patch of white light. At 62.5 (λ 6890) of the scale the red of the spectrum disappeared. As the slit moved along the spectrum, and the white was approximately reduced to equal

luminosity, she described all the red as grey, and of the same colour as the white until 53·5 (λ 6110), and after this point she said the colour was brownish compared with the white. The colour continued of this hue to her till 48 on the scale (λ 5720), when she said the colour was neither brown nor green, but both. From 48 on the scale she described the colour as green till quite sharply at 31·5 (λ 4905). In the blue she again began to see grey; the grey at this end of the spectrum, and also of the white patch, she called brownish-grey.

Scale No.	Wave-length.	Reading.	Remarks by N.W.
62·5	7019	0	Both grey.
60	6728	3	"
58	6520	10	"
56	6330	30	"
54	6152	52	Colour "brownish," white "grey."
52	5996	70	" " " "
50	5850	81	" " " "
48	5720	87	Colour "brownish-green," white "grey."
46	5596	90	Colour "green," white "grey."
44	5481	88	" "
42	5373	82	" "
40	5270	62·5	" "
38	5172	46	" "
35	5042	23	" "
32	4924	12·5	" "
31	4886	10	Colour "brownish - grey," white "brownish-green."
30·5	4862	8·5	" " " "
25	4675	5	" " " "
20	4518	3	" " " "
15	4376	2·5	" " " "
10	4248	1·5	" " " "
0	4010	0·2	" " " "

This name must evidently have been a mental distinction, as she described the red end and the white as grey only, and not brown-grey; and, indeed, she was tried again over that part of the spectrum, and adhered to the previous naming. It would appear to be due to the low luminosity which made the grey appear brownish to her, and not to any actual difference in hue.

Her curve of luminosity in the spectrum was next taken, and her readings are given in the table. The curve is shown in fig. 5. The shaded band beneath it applies to her curve. My own readings were $1/1.375$ of the normal curve as shown in the diagram. The extinction of a gas-light, in my own case and that of Mr. Nettleship, was $13^{\circ}5$. That of N. W. was 16° , showing that her final perception of light was $13.5/16$ of what we may call the normal.

An endeavour was made to form a series of colour equations with her eyesight by placing three slits in different parts of the spectrum, but without success, although a match with white was made in two positions. One slit was placed in the orange-red at about 52 of the scale, another at E, and the third at G, and white light was formed, though her match was so erratic that it was useless to measure the apertures. When the slit in the violet was covered up, a white patch being alongside as a comparison, she called the mixture of red and green "brownish-green;" when the slit in the red was covered she called the mixed light of green and violet "green;" and when the green slit was covered up she called the purple colour a "different kind of brown."

When the first slit was moved into the red near the lithium line she called the colours "green," whenever the green slot was uncovered. A piece of signal-red glass (London, Brighton, and South Coast Railway) was placed in the white reflected beam, forming a red patch, and a patch of the blue scale at No. 30.5 (λ 4862) was placed alongside, and she matched them in luminosity and in colour. (The dominant colour of the signal glass in question was λ 6220.) She finally was tested with colour discs:—

One being in red with dominant wave-length	..	λ 6150
Another, emerald-green " "	..	λ 5373
And the third, French ultramarine "	..	λ 4700.

To make white she required

$$130\text{ G} + 113\text{ R} + 117\text{ U} = 72\text{ W} + 288\text{ B (corrected).}$$

She was then tried with the blue and green discs alone and made a match—

$$258\text{ U} + 102\text{ G} = 65\text{ W} + 295\text{ B (corrected).}$$

An attempt was made to match with the green and red discs alone, but this failed.

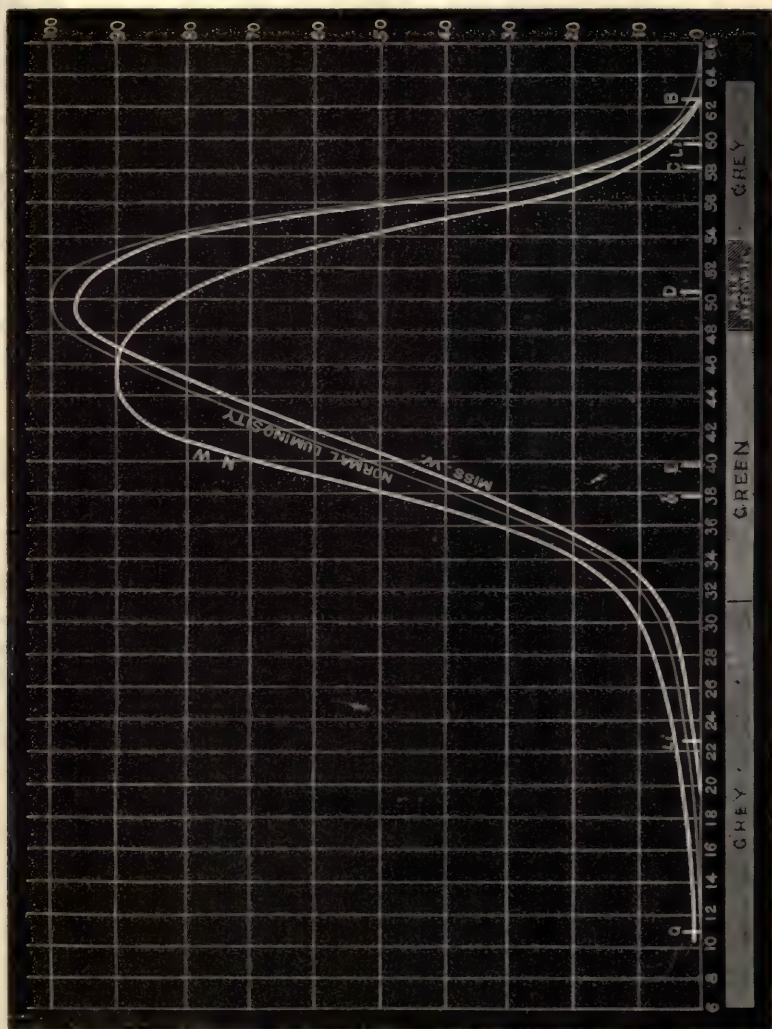
She matched the red disc alone with black and white, and also the blue disc alone—

$$360 R = 56 W + 304 B \text{ (corrected),}$$

$$360 U = 60 W + 300 B \text{ (corrected).}$$

With any proportion of R and U mixed together she matched a grey of approximately the same intensity as above, as it might be supposed she would from the last two equations.

Fig. 5.



The thin line curve is the curve of luminosity for the normal eye.

Taking the intensity curve of the light reflected from the red it was found to contain a great deal of the part of the spectrum which she called brownish, viz., from 53·5 to 48 on the scale, whereas the blue reflected a trifle of this portion of the spectrum, as did also the green, and this may account for her making a match to grey of U and G, and not of R and G, but it is hard to see why she matched U alone and also R with the grey.

Reviewing the case, it seems that any perception of colour is very small, and that the sensations are green and red, together with white. Experiments which I have described in my book on "Colour Measurements and Mixture," 1891, show that a large proportion of colour may be mixed with white without being perceived, but this colour so hidden has still the capability of neutralising a certain quantity of the complementary colour thrown on the white, which, by itself, would not be masked by the white. It would seem then that in N. W.'s case the two colours perceived were very much diluted, and at parts of the spectrum so diluted as not to be perceived, but that the latent colour, if it may be so called, has the power of forming a grey with the green which she sees more strongly.

Case VI.—This case is that of Miss W., who was brought before the Committee by Dr. Lindsay Johnson, on April 29. The right eye was apparently normal for colour, but with the other she saw nothing but shades of white.

Miss W., it appears, has had a slight stroke of paralysis, which affected her left side, and subsequently she discovered that colour sensation in the left eye had disappeared. Mr. Brudenell Carter, the day after the meeting of the Committee, examined her and pronounced hers to be a case of atrophy of the optic nerve.

I examined her with the spectrum colours on the 5th May, and found her left eye totally blind to every colour, though her perception of light was very fair. She had very little difficulty in comparing the luminosity of the most brilliant spectrum colours with the white patch of light placed alongside them. In making the measurements she experienced a certain amount of fatigue, but, by resting the eye for short intervals, her readings were very constant. The following is the table of her readings:—

Scale No.	Wave-length.	Readings.	Remarks by Miss W.
63	7082	0	Both colour and white patch appeared as white throughout the spectrum.
62	6957	1	
60	6728	7	
58	6520	18	
57	6423	28	
56	6330	43	
54	6152	76	
52	5996	90	
50	5850	95	
48	5720	93	
46	5596	83	
44	5481	71	
42	5321	58	
40	5270	46	
38	5172	32	
36	5085	21	
34	5002	12·5	
32	4924	7	
30	4848	4·5	
28	4776	3·0	
25	4675	1·5	
20	4518	0·4	
19	4488	0·0	

At 19 the light perception was so diminished that she could not match the grey. Her light perception extended further into the violet (as white) beyond this point, as the subsequent measures show conclusively.

It seemed that it would be interesting to examine her eye for the extinction of light by the same method as that described in my recent paper.

The orange sodium light of the spectrum was thrown on the apparatus therein described, of a luminosity of an amyl lamp 1 foot off, and the slit giving this brightness remained unchanged throughout the examination, and was moved through the spectrum till a position was reached where all light was just extinguished. Her perception of the point of extinction was very acute. Rotating sectors were placed in front of the apparatus, as described in the paper referred to, set at different angles, so that the amount of reduction of the luminosity of the spectrum was known at once.

Scale readings of light extinction.

Light coming through the slit reduced to—	Slit moved towards the violet.	Slit moved towards the red.
No reduction.	15	53·7
$\frac{1}{2}$ intensity.	20·7	52·4
$\frac{1}{4}$ "	21·7	50·9
$\frac{1}{8}$ "	23·2	48·7
$\frac{1}{16}$ "	26·7	46·7
$\frac{1}{32}$ "	34·7	44·2
$\frac{1}{64}$ "	—	40·0

The extinction of light with the full aperture to myself was at 2·1 and 57·9. At 57·9 the luminosity of the spectrum is 0·22 that at the D line, and as the light on the screen at the end of aperture is $\frac{1}{620}$ that falling on the instrument originally, it follows that the extinction to myself was when the light of 57·9 (λ 6510) was $0\cdot22/620 = 0\cdot000355$ of an amyl lamp placed at 1 foot from the screen. These details are given to show that the newer instrument used in these tests gives the same results as the older one, for with the latter it was 0·000350.

The place in the spectrum where Miss W. last perceives light is the same as my own. The luminosity which is invisible to her is when $1\cdot45/100,000$ of an amyl lamp illuminates a screen 1 foot off. At D if $71/100,000$ of an amyl lamp illuminated a screen 1 foot off it is invisible to her. With my own vision if the screen be illuminated with $7/100,000$ of the same light it just becomes invisible. There is therefore a marked difference between the two sights as regarding initial perception of light.

II. "On the Limit of Visibility of the different Rays of the Spectrum. Preliminary Note." By Captain W. DE W. ABNEY, C.B., R.E., D.C.L., F.R.S. Received April 29, 1891.

In certain photometric experiments it became necessary to find the limit of visibility of the different parts of the spectrum, and also to ascertain what ratio this limit would bear to some fixed luminosity. It should be borne in mind that this question is totally different from acuteness of vision, which some have confounded with it. The two are independent one of the other, and can scarcely be compared.

The instrument used in these experiments was similar to that described in the note on the examination of a case of Tobacco Scotoma, &c., but the dimensions were modified:—A square tube, 3 feet long, had an aperture of 2 inches cut in its side at 2 feet 6 inches from one end, and covered over with ground glass. Within the tube, and close to the ground glass, was a mirror, which reflected the light coming through the ground glass on to the end of the tube, and if the ground glass was illuminated by any light the reflection illuminated a card placed at the end of the tube. The illumination of the card could be viewed through a circular hole at the other end of the tube, in which was fixed a smaller tube, fitting closely into the eye. If a colour patch from the spectrum was thrown on to the ground glass, evidently the card at the end of the tube would be illuminated by the colour used, and its disappearance could be effected by means of rotating sectors closing and opening at will, placed in front of the patch. This simple piece of apparatus answered its purpose most effectively.

The first point to ascertain was the ratio of illumination of the card to that of the patch thrown on the ground glass. The following arrangement was made to effect this. The end of the tube, against which the card was placed, was removed, and a card with a square hole, of $\frac{3}{4}$ -inch side, was inserted instead. This was covered on the side away from the tube with a piece of Saxe paper, and when viewed from the outside, and when illuminated by the light from the ground glass, showed as a square patch of light. Outside of this, and of double the width, but of the same height, a mask of black paper, with an oblong aperture, was placed so that the illuminated square occupied one-half of the oblong, and the other half showed no white paper. An amyl acetate lamp (0.8 of standard candle), placed at a fixed distance from this oblong, and in a line with the axis of the tube, illuminated both squares; but a red placed in proper position cast a shadow on the translucent square, allowing only the opaque white half to be illuminated. When the sectors above alluded to were placed in front of the lamp, the two

brightnesses could be equalised, and the intensities of the light transmitted passing through the paper estimated.

Now there is a ray very near D in the spectrum, whose colour is very closely, if not quite, identical with the colour of the light emitted by the burning amyl acetate, and for making the measures this ray was used. When the measure had been made, the screen, with the square aperture, was placed in the position of the ground glass, and the amyl acetate lamp placed on the side of the screen, away from the colour patch, and the rod placed in position to cast the shadow necessary. The rotating sectors were then placed between the spectrum and the screen, and the light reduced so that the illumination of the translucent and opaque white square, *viewed from the side of the lamp*, was equalised. Knowing the distance of the lamp in the two cases, and the aperture of the sectors, the relative illumination of the two surfaces was ascertained. For convenience, the aperture of the ground glass was limited by means of a diaphragm, or by placing a diaphragm in front of the first prism.

Two sets of measures showed that if the illumination of the ground glass be represented by 1, the illumination of the card at the end of the tube was $\frac{1}{100}$; that is, any light falling on the ground glass was diminished to that extent.

The actual measures were $\frac{1}{600}$ and $\frac{1}{112}$, but we may take $\frac{1}{100}$ as sufficiently close to the truth.

The colour-patch apparatus to which reference is made is described in the Bakerian Lecture, 1886 (Abney and Festing, "Colour Photometry"). The only addition to it that was made was to use an adjustable slit to move through the spectrum. There was thus a treble means of altering the intensity of the light, viz., by altering the aperture of the slit of the collimator, by altering that of the slit of the slide, which was shifted at will into different parts of the spectrum, and by the rotating sectors placed in front of the spectrum. The mode of proceeding to measure the luminosity at which light disappeared was as follows:—The dullest part of that portion of the spectrum which it was desired to extinguish was allowed to pass through the slit in the spectrum, and a patch was formed on the ground glass, which, it may be remarked, had a tube fitted over it, to prevent any chance of extraneous light reaching it. The card at the end of the square box was viewed, and the slits closed till all trace of light disappeared. (It may be as well to call to mind what is well known, that faint light of all colours appears as white.) In some sets of experiments the sectors were set at fixed angles, and rotated in front of the patch, and the slit in the spectrum moved from a position in which faint light appeared to one in which it just disappeared, the position in the spectrum being noted by the scale at the back of the moving slide carrying the slit. ²⁰⁰ In other cases the slit

was placed at different positions in the spectrum, and the rotating sectors closed till all light had vanished, when the aperture was noted. The first plan is the more convenient of the two, and gives very accurate results; though in some positions of the spectrum the second method must be adopted, since the graphic curve formed from the readings becomes almost a horizontal straight line at one portion of the spectrum. As will be seen from the table, it is quite evident that no one aperture of the slit of the collimator and of that in the slide would suffice to give the entire range of disappearance of the spectrum, and that at least three settings are necessary. At each change the D light falling on the ground glass was measured, and the necessary factors to make the readings on one scale were derived from these measurements.

Four sets of measures throughout the spectrum were made on different days. No one differed to any appreciable extent from the other. A mean of the four has been taken as representing the truth, and the measures given in the first table are those of that which most nearly approaches this mean. It may be stated that very rarely did one curve differ more than 4 per cent. from another at any portion of the spectrum. The readings were taken when the eye had rested in darkness some time, and were often repeated a considerable number of times, the first parts measured being re-measured last. That the eye was equally sensitive throughout the time may be judged from the fact that the two sets of readings scarcely ever differed. The process of making these measures of extinction is very fatiguing, and probably rather detrimental to the eyesight; owing to the strain on the eyes, one set of readings is usually as much as can be properly carried out on any one day, if accurate results are to be looked for.

It is now three years ago since I began this research, and, after trying various plans, I have come to the conclusion that the method now described is the most easy, as it is the most simple.

There is one point in the method which might be open to criticism, and that is that the cutting off the light by rotating sectors might cause some error in the results. This criticism, I may say, I raised in my own mind at its very commencement, and found that it was unnecessary. Polarising the light entering the slit of the collimator, and then dimming it by means of a Nicol's prism placed in front of the colour patch, proved an unsatisfactory method for answering the criticism, as in no case could a total disappearance of a bright light be brought about; but by diminishing the area of the colour patch by placing different apertures of diaphragms in front of the last prism of the colour-patch apparatus (and thus throwing on the ground glass discs of light of various areas), the truth of the results was readily verified. The two sets of measures, one made in this way

and the other as just described, gave identical results within the limits of the errors necessarily due to observation.

The method adopted gave the extinction of light on the whole retina, for not only was the central part used, but the extinction was carried so far that it was complete for every part of the eye. As there is a considerable absorption in the yellow spot this is necessary, but the absorption exercised in this part of the eye, which occupies from 4° to 6° angular aperture, can be fairly measured if only the light on a small area be extinguished and this part of the retina be alone used. A very simple way of seeing the absorption of the yellow spot is to form a feeble spectrum some 3 inches long on a ground-glass screen. If the eye looks at the green, a dark band ex-

Table I.

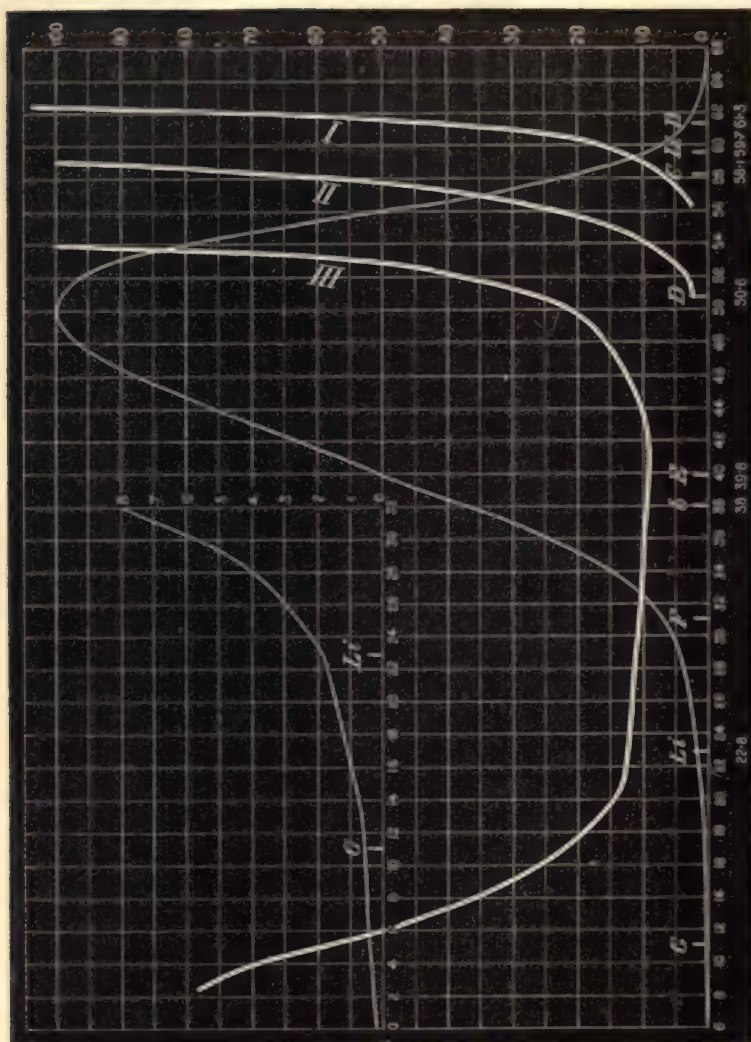
No. 1.			No. 2.		
Scale No.	Sector aperture.	Sector aperture reduced.	Scale No.	Sector aperture.	Sector aperture reduced.
55.2	180	180	57.3	180	456
5.3	"	"	2.1	"	"
54.0	90	90	55.9	90	228
9.3	"	"	4.3	"	"
53.2	60	60	54.1	38	97
10.6	"	"	8.3	"	"
52.3	45	45	53.1	20	51
13.3	"	"	12.3	"	"
51.3	32	32	Luminosity of patch on No. 2 = 2.56 that of No. 1.		
15.9	"	"			
50.5	25	25	No. 3.		
16.3	"	"			
50.0	22.5	22.5	60.8	180	2700
17.3	"	"	..	..	..
48.4	15	15	59.4	90	1350
19.3	"	"	58.3	45	675
45.4	11	11	56.9	22.5	337
26.3	"	"	53.4	5	75
D light had to be reduced to 0.17789 its luminosity to equal the light from an amyl lamp at 48 cm. from the ground glass.			Luminosity of patch No. 3 = 15 that of No. 1.		

Table I—*continued.*

No. 4.			No. 5.		
Scale No.	Sector aperture.	Sector aperture reduced.	Scale No.	Sector aperture.	Sector aperture reduced.
52·3	180	45	61·9	180	6000
14·3	"	"	60·9	90	3000
49·8	90	22·5	60·2	60	2000
17·3	"	"	59·0	30	1000
44·3	45	11·25	57·6	15	500
26·3	"	"	56·5	9	300
43·3	40	10	Luminosity of patch in No. 5 = 22·2 times that of No. 1.		
35·3	"	"			
25·3	45	11·25	A measure showed that 63 required double the aperture of 62 to be extinguished.		
30·3	43	10·75			
34·3	40	10			
38·3	37	9·2			
Luminosity of patch in No. 4 = 0·25 that of No. 1.					

tending to the blue will be seen, but if the eye be turned towards the red end or violet, the green is seen outside the central spot and the colour reappears. I propose to return to this in a fuller discussion of the subject.

The first table shows the actual observations in the spectrum.



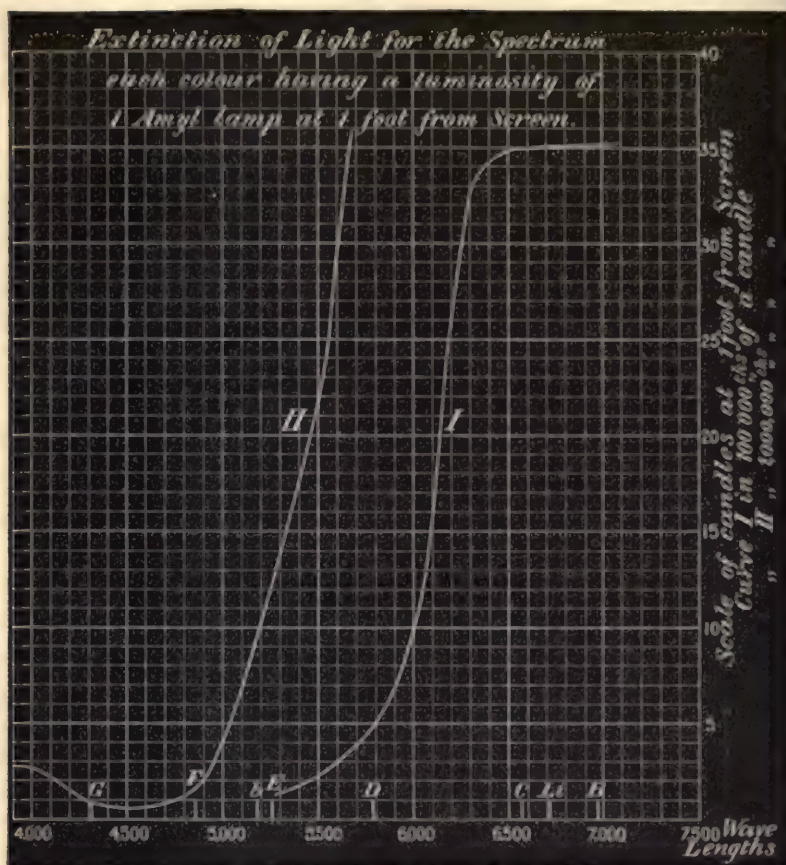
The ordinates of curve II are 10 times that of I, and of curve III 100 times that of I.

The second table attached shows the extinction of light of a luminosity of one amyl lamp placed at a foot from the screen. It

Table II.

Extinction of Rays of Equal Luminosity, the Luminosity being 1 Amyl Lamp at 1 foot from a Screen.

Scale No.	λ .	Reading.	Luminosity of rays.	Extinction of equal luminosities.	$\frac{1}{100000}$ of an amyl lamp 1 ft. off screen.
63	7082	13,000	1	13,000	36.11
62	6957	6,400	2	12,800	35.5
61	6239	3,100	4	12,400	34.4
60	6728	1,800	7	12,600	35.0
59	6621	1,000	12.5	12,500	34.7
58	6520	600	21	12,600	35.0
57	6423	380	33	12,540	34.8
56	6330	240	50	12,000	33.3
55	6242	160	65	10,400	29.0
54	6152	100	80	8,000	22.2
53	6074	55	90	4,950	13.75
52	5996	38	96	3,640	10.11
51	5919	28	99	2,772	7.70
50	5850	21	100	2,100	5.83
49	5783	17	99	1,682	4.65
48	5720	16	97	1,552	4.31
47	5658	14	92.5	1,294	3.59
46	5596	12.4	87	1,078	2.99
45	5535	11.6	81	906	2.517
44	5481	10.0	75	750	2.083
43	5427	9.8	69	686	1.905
42	5373	9.6	62.5	600	1.666
41	5321	9.6	67	546	1.516
40	5270	9.6	50	480	1.333
38	5172	9.6	36	346	0.911
36	5085	9.8	24	236	0.655
34	5002	10.0	15	150	0.4166
32	4924	10.2	8	82	0.2277
30	4845	11.0	5.5	50	0.1388
28	4776	11.2	4	43.6	0.1166
26	4707	11.6	3	35	0.0972
24	4643	12.0	2.2	26.4	0.0733
22	4578	12.4	1.6	20	0.0555
20	4519	14.0	1.4	18	0.0500
18	4459	18	1.2	21.6	0.0600
16	4404	26	0.9	23.4	0.0650
14	4349	36	0.7	25.2	0.0700
12	4298	50	0.6	30	0.0833
10	4247	70	0.55	36.4	0.1011
8	4197	104	0.5	52	0.1444
6	4151	160	0.4	64	0.1777
4	4105	240	0.35	84	0.2333
2	4058	350	0.3	105	0.2916
0	4010				



will be seen that the extinction of the red rays is effected when they are reduced to about $36/100,000$ of this standard, whilst the rays near F require a reduction of $5/10,000,000$, that is, the sensitiveness of the eye is 700 times greater for the latter colour than the former, and this has a bearing on the extinction of white light of different qualities.

It is worthy of remark that the reduction of the rays from about C to the visible limit of the red necessary to cause extinction from the standard luminosity is practically the same, and points to the fact that this part of the spectrum is probably monochromatic; if admixture of any other colour sensation were present, the curve would rise or fall instead of remaining horizontal. The same apparently applies to the violet end of the spectrum, though, owing to the small luminosity, exact measures of it are less certain. The experiments show that the rays having the wave-length of about $\lambda 4770$ are the last perceived. The shift in the position of maximum resistance to

about λ 4510, as shown in Table II, is due to the fact that equal luminosities of each colour have been considered as being reduced.

Some interesting experiments were carried out by placing slits in different parts of the spectrum, and forming a mixture of light on the ground glass of the apparatus. An intense D light mixed with a faint light near F formed a colour patch, and this mixed light was extinguished and found to require 9° of aperture of the sector. The D light was then shielded and the single ray of blue-green light was extinguished, when it was found that the same aperture was required to extinguish this beam alone. Red and green and various other mixtures were tried, all showing that in the extinction of light the green-blue light was the last visible, and was equivalent to extinguishing that light alone, although it might be mixed with very much brighter light in the red or yellow. In the blue the conditions somewhat change, as will be seen in the diagram, but if slits of equal aperture were used the same results were obtained.

The diagram shows that in the spectroscopy of feeble light the rays in the blue and green are the first to be perceived, and that rays of far greater intensity in the yellow and red may exist without exciting the sense of light. This may account for some of the varied results recorded in eye spectroscopic observations of sources of feeble luminosity, in which the yellow and red lines are absent.

In extinguishing white light, the fact of the total extinction of the blue-green light is of importance.

It is not the *light* at that particular wave-length which disappears last, but some one *sensation* which is principally existent at that point, but which extends over a great portion of the spectrum which has to be extinguished. For instance, in extinguishing the light from the reflected beam of the electric light already alluded to, it was found that the light illuminating the ground glass was 720 times brighter than that reaching the screen. To extinguish 0.014 of the light from an amyl lamp on the ground glass the sector had to be closed to 21, that is the light of one amyl lamp luminosity, falling on the screen at 1 foot distance, had to be reduced to

$\frac{14}{1000} \times \frac{1}{720} \times \frac{21}{180}$ or $\frac{1}{441,000}$ of the original light. Had the luminosity of the unit of luminosity been due entirely to the colour at λ 4776, it would have had to be reduced to about $\frac{1}{900,000}$ of its luminosity before it became invisible. Thus the electric light gives about half the sensation of this light that the monochromatic light of that colour and luminosity would give, and hence we may conclude that about half the luminosity of the white light is due to this sensation, of course distributed unequally through its spectrum. This is a very close approach to the area of the green sensation curve of the spectrum when the luminosity is taken into account.

It would thus appear that by studying the extinction curves it may be possible to approximate to the three positions in the spectrum which the colours giving the nearest approach to the three fundamental sensations on the Young-Helmholtz theory occupy.

III. "Researches on the Structure, Organisation, and Classification of the Fossil Reptilia. VII. Further Observations on *Pareiasaurus*." By H. G. SEELEY, F.R.S., Professor of Geography in King's College, London. Received May 5, 1891.

(Abstract.)

The author distinguishes five zones of life in the Karoo rocks, which are termed, counting from the bottom, Mesosaurian, Pareiasaurian, Dicynodont, Theriodont, and Zancloodont. The Pareiasaurian zone extends between the Prince Albert Road station and the Nieuwveldt range of mountains. He obtained a nearly complete skeleton from Bad, east of Tamboer, a less complete skeleton from Tamboer Fontein, and a portion of jaw from near Klipfontein, on the summit of the Nieuwveldt range. These materials show almost every part of the skeleton except some details of the carpus and tarsus, and the number of-digits.

The skull shows in both specimens the structure of the palate, which was closed in the median line, and almost covered with teeth, which extend in four principal longitudinal rows on the vomera and pterygoids. The teeth are slender, cylindrical, and recurved. There are two oblique rows, half as long as the others, on the palatines. They converge backward. Other teeth occur in rows behind these, and in front of them. The posterior nares open behind the pterygoids on the basi-sphenoid. The pterygoid bones diverge backwards to meet the quadrate bones, which are wedged in between them and the bones of the cheek. On the outer border of the side of the quadrate is a perforation like that figured 'Phil. Trans.,' B, 1889, Pl. 10, fig. 4, only smaller. The brain case has the same sort of relation to the roof bones of the skull, as in marine Chelonia. The brain case is depressed behind. The occipital condyle appears to be formed by the basi-occipital in its lower half, and by the ex-occipitals in its upper half. It is concave, and was margined below by a semi-circular intercentral bone. A similar intercentral ossification occurs behind it, below the atlas. The surface of the skull has no opening except the nares, orbits, and the large parietal foramen. Its posterior border is concavely notched. The surface shows the same pitted and channeled ornament as in the specimen already described.

The vertebral column is complete with the exception of a few small terminal vertebræ of the tail. No neural arch has been found to the

first vertebra. The processes for articulation with the dorsal ribs have elongated facets, which are rarely divided into diapophyses and parapophyses. The sacrum includes four vertebræ, of which the first is sacro-lumbar and the last two sacro-caudal. Chevron bones are well developed along the tail.

The shoulder girdle is placed far forward; the precoracoid, coracoid, and scapula are anchylosed together. The scapula is expanded and elongated, extending backward towards the ilium. The clavicular arch includes five bones. The interclavicle has a descending median bar, which expands transversely between the coracoids; its transverse bar unites with the clavicles, which rest upon the scapulæ. They only extend half-way along the length of the superior margins of the scapulæ. Beyond that point is another pair of bones which represent the supraclavicles, as in Fishes and Labyrinthodonts.

The pelvis is entirely Mammalian in form. The pubes are almost entirely behind the iliac bones, and unite with the ischia to form a continuous sheet of bone, the two sides being inclined to each other and meeting in a ventral symphysis. There is only a small perforation through the pubis, and no perforation between the pubis and ischium, as in Mammals. The transverse processes from the four sacral vertebræ meet the expanded blade of the ilium along its length on each side.

The limbs are massive and short; the femur shows characters which have previously been regarded as belonging to the humerus. The distal end of the bone is perforated. The lesser trochanter is strongly developed. The tibia is large and massive, and the fibula slender. These bones are much shorter than the femur. The os calcis is of large size, and articulates with both the fibula and tibia; the astragalus is small. The tarsal bones of the distal row are small and separate; their relations to each other not definitely determined. The metatarsal bones are strong and short; the phalanges are short, and terminate in massive, long, flattened claws. In the fore-limb the humerus is greatly expanded at both ends with a large deltoid crest. The condyles of its distal end are well rounded; the radius is short and massive; the ulna expands at its proximal end, and is produced according to the Mammalian plan so as to receive the distal end of the humerus. The carpus is imperfectly known. The digits were stronger than those of the hind limb, and terminated in similar claws.

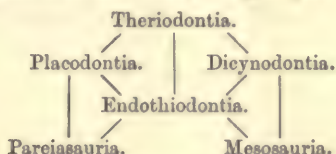
The specimens show that in characters of the teeth and mandible there is nothing to distinguish *Anthodon* from *Pareiasaurus*; and that the genus *Propappus* apparently has no existence, being founded on a femur. One species is named *Pareiasaurus Bainii*, another is *Pareiasaurus Russauwi*.

All the affinities hitherto attributed to *Pareiasaurus* with Labyrinthodonts, Anomodonts, *Procolophon*, and Mammals are shown more

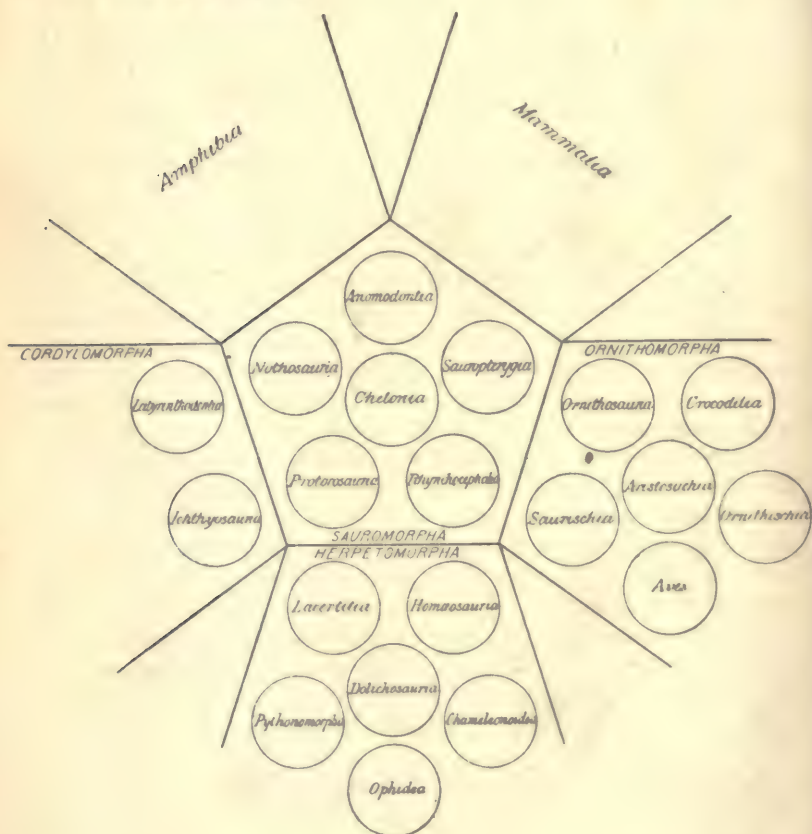
strongly in the several parts of the skeleton, by the new evidence. The shoulder girdle is more Labyrinthodont than was previously supposed, the skull is more Reptilian, and the pelvis and limbs are more Mammalian, though with some resemblance to Dinosaurs.

From further evidence of the structure of the skeleton in *Procolophon*, the author regards that type as a member of the Pareiasauria, rather than as forming a distinct sub-order. It also has four sacral vertebrae.

The divisions of the Anomodontia are grouped as—



The relations of the Anomodontia to other Vertebrata are expressed in the following grouping :—



IV. "On the Theory of Electrodynamics." By J. LARMOR, Fellow of St. John's College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received May 11, 1891.

The electrical ideas of Clerk Maxwell, which were cultivated partly in relation to mechanical models of electrodynamic action, led him to the general principle that electrical currents always flow round complete circuits.

To verify this principle for the case of the current which charges a condenser, it was necessary to postulate an electrodynamic action of the same type as that of a current for the electric displacement across the dielectric, in which the excitation of the dielectric may be supposed, after Faraday, to consist. The existence of such an action has subsequently been deduced qualitatively from the general principle of action and reaction,* and has also been detected by various experimenters.

The principle also requires that the electric displacement shall not lead to any accumulation of charge in the interior of the dielectric, therefore that it shall be solenoidal or circuital,† its characteristic equation being of the type

$$\frac{d}{dx} \left(K \frac{dV}{dx} \right) + \frac{d}{dy} \left(K \frac{dV}{dy} \right) + \frac{d}{dz} \left(K \frac{dV}{dz} \right) = 0,$$

where V is the electric potential, and K a dielectric constant. The surface density of the electricity conducted to a face of a condenser must neutralise the electric displacement, and not leave any residual effective electrification on the surface. On taking the displacement and the surface density each equal to $KF/4\pi$, where F denotes the electric force, the value of K becomes unity for a vacuum dielectric; and K represents the specific inductive capacity as measured by electrostatic experiments.

When this principle of circuital currents is postulated, the theory of electrodynamics is reduced to the Ampère-Neumann theory of complete circuits, of which the truth has been fully established. It leads, as shown by Maxwell, to the propagation of electrical action in dielectric media by waves of transverse electric displacement, which have the intimate relations to waves of light that are now well known.

* Cf. J. J. Thomson, 'Brit. Assoc. Report,' 1885.

† A term recently introduced by Sir W. Thomson.

Generalised Polarisation Theory.

The problem of determining how far these remarkable conclusions will still hold good when a more general view of the nature of dielectric polarisation is assumed was considered by von Helmholtz* in a series of memoirs.

The most general conception of the polarisation of a medium which has been formed is the Poisson theory of magnetisation. The magnetised element, whether actually produced by the orientation of polar molecules or otherwise, may be mathematically considered to be formed by the displacement of a quantity of ideal magnetic matter from its negative to its positive pole, thereby producing defect at the one end, and excess at the other end. The element is defined magnetically by its moment, which is the product of the displaced quantity and the distance through which it is displaced. The displacement per unit volume, measured by this product, is equal to the magnetic moment per unit volume, whether the magnetised molecules fill up the whole of that volume or are a system of discrete particles with unoccupied space between them.

In the electric analogue we replace ideal magnetic matter by ideal electric matter; the displacement thus measured constitutes the electric displacement, and its rate of change per unit time represents the displacement current in the dielectric. We have to consider whether a displacement current of this type suffices to make all electric currents circuital; and it will be sufficient and convenient to examine the case of a condenser which is charged through a wire connecting its two plates. In the first place this notion of electric displacement leads to the same distribution of potential between the plates as the ordinary one, adopted by Maxwell; for in the theory of induced magnetism there occurs a vector quantity of circuital character, the magnetic induction of Maxwell, of which the components are $-\mu(dV/dx)$, $-\mu(dV/dy)$, $-\mu(dV/dz)$, and which, therefore, leads to the characteristic equation of the potential

$$\frac{d}{dx} \left(\mu \frac{dV}{dx} \right) + \frac{d}{dy} \left(\mu \frac{dV}{dy} \right) + \frac{d}{dz} \left(\mu \frac{dV}{dz} \right) = 0,$$

corresponding to the one given above. If the displacement in the dielectric is $-\kappa(dV/dx)$, $-\kappa(dV/dy)$, $-\kappa(dV/dz)$, then

$$\mu = 1 + 4\pi\kappa.$$

The displacement in a unit cube may, of course, be considered as a displacement across the opposite faces of the cube.

Now, considering the case of a plane condenser, let F be the electric force in the dielectric between the plates; then the displacement is

* 'Wissenschaftliche Abhandlungen,' I, p. 545, *et seq.*

κF . Let σ be the surface density of the charge conducted to a plate ; then the effective electrification along that plate will be of surface density $\sigma' = \sigma - \kappa F$; therefore, by Coulomb's principle,

$$F = 4\pi\sigma'$$

$$= 4\pi(\sigma - \kappa F) ;$$

so that

$$\sigma = \frac{\mu}{4\pi} F = \kappa F + \frac{1}{4\pi} F.$$

Thus the current is not circuital, but there is an excess of the surface density conducted to the surface over the displacement current from the surface, which is equal to $F/4\pi$.

The specific inductive capacity, as determined by static experiments on capacity, is here measured by μ , the coefficient in the expression for σ .

In addition to this discontinuity at the face of a condenser plate, the induction in the mass of the dielectric will not be circuital unless the electric force is itself circuital, which it is not in the general electrodynamic theory to be presently discussed.

The current becomes more nearly circuital the greater the value of μ . If μ , and therefore κ , were infinite we should attain the limit when the currents are circuital. If the values of μ for all dielectrics were multiplied by the same infinite constant, so as to keep their ratios unchanged, the distribution of electric potential would not be altered, provided the charges on all conducting surfaces were also increased in that ratio ; the displacement or induction, which is now the essential quantity in the theory, thus maintaining its original value. This comes to the same thing as measuring the actual charges in a unit which is diminished in that ratio.

In this way the Maxwell scheme of circuital currents reveals itself as a limiting case of the more general polarisation theory. The infinite dielectric constant makes the excited polarisation of very great amount in comparison with the exciting cause ; so that in the limit we may, in a sense, imagine the system as one of self-excited circuital polarisation, a point of view which approaches somewhat to that of Maxwell himself.

This mode of connecting the two theories was pointed out by von Helmholtz. But his scheme takes for the new unit of charge the electrostatic unit corresponding to vacuum with its new infinitely great dielectric constant, so that this unit is reduced proportionally to the square root of the infinite ratio ; the displacement is then infinitely great, and the potential infinitely small, according to the square root of this ratio.

(This, however, should be expressed more precisely as follows :—

The *absolute* dimensions of electric charge and electric displacement in K are both $K_2^{\frac{1}{2}}$, those of electric force (static) $K_2^{-\frac{1}{2}}$. These dimensional relations must persist when the transition is made from von Helmholtz's system to Maxwell's, so that the changes in the units are as von Helmholtz indicates; and the ratio of the electrostatic to the electrokinetic unit of quantity in an ideal absolute medium with K_2 unity will now be the ascertained value of this constant for air or vacuum multiplied by the square root of the value of K_2 for air. The electric pressure in a fluid dielectric, however, depends, in this limiting form of the theory, on the square of the value of the electric displacement, as may be proved: thus the circumstances of ordinary cases of statical electrification are those of finite numerical value of the displacement, notwithstanding the smallness of this absolute unit of charge.)

Generalised Electrodynamic Theory.

To obtain the general type of the modification of which the theory of electrodynamics is susceptible owing to the existence of non-circuital currents, we start, following von Helmholtz, from the ascertained laws for circuital currents, which may be developed in the manner of Neumann and Maxwell from the electrodynamic potential

$$T = u' \iint \frac{\cos \epsilon}{r} ds ds'.$$

The value of T with the sign here given to it is to be reckoned as kinetic energy; the mechanical forces are to be derived by its variation due to any virtual displacement of the system, a force acting in the direction of the displacement producing an increment of T ; the electric forces are derived according to Lenz's law or Maxwell's kinetic theory. The equations of the field are thus all expressible in terms of this function T . When non-circuital currents are contemplated, the currents i, i' , now varying with s, s' , must be placed inside the integral signs; and to T must be added the most general type of expression that will vanish when either current is circuital. Thus we must write

$$T = \iint i ds i' ds' \left(\frac{\cos \epsilon}{r} + \frac{d^2 \Psi}{ds ds'} \right),$$

where Ψ is a function such that

$$\int \frac{d\Psi}{ds} ds = 0, \quad \int \frac{d\Psi}{ds'} ds' = 0,$$

i.e., it is, so far, any function which has no cyclic constant round either circuit. The distribution of the energy between the pairs of

elements is now supposed to be specified by the elements of this integral.

The form of Ψ is limited by the fact that it must be a function of the geometrical conformation of the pair of elements. The elements of this conformation are given by the equations

$$\cos \theta = -\frac{dr}{ds},$$

$$\cos \theta' = \frac{dr}{ds'},$$

$$\begin{aligned}\cos \epsilon &= -\frac{d}{ds'}\left(r\frac{dr}{ds}\right) \\ &= -\frac{dr}{ds}\frac{dr}{ds'} - r\frac{d^2r}{ds ds'}\end{aligned}$$

where r is their distance apart, $\theta, \theta', \epsilon$ represent the angles $r.ds, r.ds', ds.ds', r$ being measured positive from ds to ds' .

The only function of the type $d^2\Psi/ds ds'$ which can be specified in terms of these quantities is $d^2\phi(r)/ds ds'$, which is equal to

$$r^{-1}\phi'(r)(\cos \theta \cos \theta' - \cos \epsilon) + \phi''(r) \cos \theta \cos \theta'.$$

On substitution we have

$$T = \iint ds' ds' \left\{ -\frac{1}{r} \frac{dr}{ds} \frac{dr}{ds'} - \frac{d^2(r-\phi r)}{ds ds'} \right\},$$

in which the elements of the energy are supposed to be correctly localised.

To obtain the mutual mechanical forces between the conductors we have to determine the variation in T produced by the most general virtual displacements of the separate elements which do not alter these elements, nor break the continuity of either circuit. Thus ds, ds', ι, ι' are not to be varied.

The shortest way to take account of currents which are not of the same strength all along the circuit is to consider two uniform currents ι, ι' flowing in interrupted circuits, and examine the terms of the variation involving the terminal points at which electric charges are being accumulated by the currents flowing into them. Of course the same general results would flow from taking ι, ι' functions of s, s' respectively and neglecting the ends. Thus, employing electromagnetic units and so avoiding a numerical coefficient, we have, after F. E. Neumann and von Helmholtz,

$$\begin{aligned}
T &= \iint ds \, i' ds' \left\{ -\frac{1}{r} \frac{dr}{ds} \frac{dr}{ds'} - \frac{d^2(r-\phi r)}{ds \, ds'} \right\}; \\
dT &= \iint ds \, i' ds' \left\{ \frac{\partial r}{r^2} \frac{dr}{ds} \frac{dr}{ds'} - \frac{1}{r} \frac{d\hat{e}r}{ds} \frac{dr}{ds'} - \frac{1}{r} \frac{dr}{ds} \frac{d\hat{e}r}{ds'} - \frac{d^2\hat{e}(r-\phi r)}{ds \, ds'} \right\} \\
&= -\int \left| \partial r \right|_{s_1}^{s_2} i' ds' \left\{ \frac{1}{r} \frac{dr}{ds'} - \int \left| \partial r \right|_{s_1'}^{s_2'} ds i' \frac{1}{r} \frac{dr}{ds} \right. \\
&\quad \left. + \iint ds \, i' ds' \, \partial r \left\{ \frac{1}{r^2} \frac{dr}{ds} \frac{dr}{ds'} + \frac{d}{ds} \left(\frac{1}{r} \frac{dr}{ds'} \right) + \frac{d}{ds'} \left(\frac{1}{r} \frac{dr}{ds} \right) \right\} \right. \\
&\quad \left. - \left\| u'(1-\phi'r) \partial r \right\|_{s_2}^{s_1} \right\}_{s_1'}^{s_2'}.
\end{aligned}$$

This variation is accounted for by the following forces of repulsion, tending to increase r .

(i) Between the elements ds and $i' ds'$, equal to

$$ds \, i' ds' \left(-\frac{1}{r^2} \frac{dr}{ds} \frac{dr}{ds'} + \frac{2}{r} \frac{d^2 r}{ds \, ds'} \right),$$

or $-2ds \, i' ds' \frac{1}{r^2} (\cos \epsilon - \frac{2}{3} \cos \theta \cos \theta'),$ Ampère's law.

(ii) Between the element ds and the positive end of the conductor ds' ,

$$ds \, i' \frac{1}{r} \frac{dr}{ds},$$

or $-ds \frac{de'}{dt} \frac{1}{r} \cos(r \cdot ds),$

where de'/dt is the rate at which the charge at that end is increasing.

(iii) Between the element $i' ds'$ and the end of the conductor ds ,

$$i' ds' \frac{1}{r} \frac{dr}{ds},$$

or $i' ds' \frac{de}{dt} \frac{1}{r} \cos(r \cdot ds'),$

r being here measured away from ds' .

(iv) Between an end of one conductor and an end of another conductor,

$$-u'(1-\phi'r),$$

or $-\frac{de \, de'}{dt \, dt} (1-\phi'r).$

It is to be observed that the form of $\phi(r)$ affects only the forces (iv) in this scheme of attraction, as one would expect from the fact that $\phi(r)$ disappears if either current flows round a complete circuit.

Not to refer to (ii) and (iii), we notice from (iv) that two changing electrifications attract each other with a force involving a term which is constant at all distances, unless a special form of $\phi(r)$ be assigned differing from any of the values which occur in the sequel. It is difficult to imagine the mechanical basis of such an action; the remarks of von Helmholtz in justification (against Bertrand) may, however, be referred to.*

This investigation of the mechanical forces is equivalent to von Helmholtz's with the exception that he takes at the beginning $\phi(r)$ to be proportional to r , on the general ground that the potential energy of two elements in all natural actions involves only the inverse first power of the distance. The validity of this consideration seems to be weakened by the fact noticed above that $\phi(r)$ occurs only in the force (iv). For what follows it will not be necessary to restrict the form of $\phi(r)$.

To discuss the propagation of electrical action in continuous media, we have to translate T from the form suitable to linear distributions to the form suitable to volume distributions. Following the method first developed by Kirchhoff, and for this case the analysis of von Helmholtz, the energy function for any field of currents is to be obtained by summation of the energy functions of all the pairs of elementary filaments of currents that compose it, care being taken that no pair is counted twice over. The proper form will be a volume integral; instead of ds, ds' , the elements of the filament, it will involve $d\tau, d\tau'$, the elements of volume, and instead of i, i' , the resultant currents, it will involve their components per unit sectional area uvw and $u'v'w'$.

$$\begin{aligned} \text{Thus } T &= \iint \frac{ds \, i' \, ds' \cos \epsilon}{r} + \iint i \frac{d}{ds} i' \frac{d}{ds'} \phi(r) \, ds \, ds' \\ &= \frac{1}{2} \iint \frac{1}{r} (uu' + vv' + ww') \, d\tau \, d\tau' \\ &\quad + \frac{1}{2} \int d\tau \left(u \frac{d}{dx} + v \frac{d}{dy} + w \frac{d}{dz} \right) \int d\tau' \left(u' \frac{d}{dx'} + v' \frac{d}{dy'} + w' \frac{d}{dz'} \right) \phi(r), \end{aligned}$$

the factors $\frac{1}{2}$ being inserted because the volume integrals, being extended all over the system, take each pair of elements twice over.

Hence

$$T = \frac{1}{2} \int (Fu + Gv + Hw) \, d\tau + \frac{1}{2} \int \left(u \frac{d\chi}{dx} + v \frac{d\chi}{dy} + w \frac{d\chi}{dz} \right) d\tau,$$

* 'Wissen. Abhandl.,' I, p. 708.

where

$$F = \int \frac{u'}{r} d\tau', \quad G = \int \frac{v'}{r} d\tau', \quad H = \int \frac{w'}{r} d\tau',$$

$$\chi = \int \left(u' \frac{d\phi}{dx'} + v' \frac{d\phi}{dy'} + w' \frac{d\phi}{dz'} \right) d\tau';$$

in these formulæ the accents may now be dropped, as the integrals are extended over the whole system.

It is through this function χ that the indeterminateness enters into the equations of electrodynamics. In a certain class of cases the function may be expressed in another form, which is useful in the subsequent analysis. By integration by parts throughout space, we obtain

$$\begin{aligned} \chi &= \int dS \phi (lu + mv + nw) - \int d\tau \phi \left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} \right) \\ &= \int d\tau \phi \frac{d\rho}{dt}, \end{aligned}$$

provided we can neglect the surface integral over the infinite sphere; and this we can do, if the system is confined to a finite region and ϕ contains only inverse powers of r , or it may be direct powers of r when there is no total current flow to infinity. Thus

$$\begin{aligned} \chi &= -\frac{1}{4\pi} \int d\tau \phi \nabla^2 \frac{dV}{dt} \\ &= -\frac{1}{4\pi} \int d\tau \frac{dV}{dt} \nabla^2 \phi, \end{aligned}$$

by Green's theorem, provided the surface integrals vanish as before.

In this equation,

$$\nabla^2 \phi = \left(\frac{d^2}{dr^2} + \frac{2}{r} \frac{d}{dr} \right) \phi;$$

so that, on von Helmholtz's assumption

$$\phi(r) = Cr,$$

we have

$$\nabla^2 \phi = 2C/r;$$

and therefore

$$\chi = -\frac{1}{4\pi} \int d\tau \frac{2C}{r} \frac{dV}{dt}$$

so that

$$\nabla^2 \chi = 2C \frac{dV}{dt},$$

a result to be used immediately.

The final result is

$$T = \frac{1}{2} \int d\tau (F_1 u + G_1 v + H_1 w),$$

where $F_1 = F + \frac{d\chi}{dx}, \quad G_1 = G + \frac{d\chi}{dy}, \quad H_1 = H + \frac{d\chi}{dz},$

$$\begin{aligned} \chi &= \int d\tau \left(u \frac{d\phi}{dx} + v \frac{d\phi}{dy} + w \frac{d\phi}{dz} \right) \\ &= \frac{1}{4\pi} \int d\tau \frac{dV}{dt} \left(\frac{d^2\phi}{dr^2} + \frac{2}{r} \frac{d\phi}{dr} \right). \end{aligned}$$

To obtain the components PQR of the electric force we assume, following F. E. Neumann and von Helmholtz, that the principle involved in Lenz's law is applicable to the element as well as to the circuit as a whole. This is the same principle as flows from Maxwell's dynamical theory, and is justified, if we assume that T is the energy function of an actual dynamical system. To the kinetic part of the electric force so determined the electrostatic part must be added, giving in all the components

$$P = -\frac{dF_1}{dt} - \frac{dV}{dx}, \quad Q = -\frac{dG_1}{dt} - \frac{dV}{dy}, \quad R = -\frac{dH_1}{dt} - \frac{dV}{dz}.$$

The conduction current is given by

$$\sigma(u, v, w) = (P, Q, R),$$

where σ is the specific resistance. The total current is

$$(u, v, w) = \left(u_1 + \frac{df}{dt}, v_1 + \frac{dg}{dt}, w_1 + \frac{dh}{dt} \right),$$

where $(f, g, h) = \frac{K_1}{4\pi} (P, Q, R).$

The vector potential FGH is connected by definition above with uvw by the equations of potential

$$\nabla^2 (F, G, H) = -4\pi (u, v, w);$$

while the characteristic equation of V is

$$\begin{aligned} \nabla^2 V &= -4\pi\rho \\ &= 4\pi \left(\frac{df}{dx} + \frac{dg}{dy} + \frac{dh}{dz} \right). \end{aligned}$$

The equations of electric propagation are involved in these results.

The value of K_1 in electromagnetic units is very small, the square of the reciprocal of the velocity of light in the medium; so that there are, broadly, two classes of media, (i) conductors in which K_1 is neglected, (ii) insulators in which u, v, w are zero. The equations of propagation for each case are involved in the above equations.

Propagation in Dielectric Media.

The simplest and most important case of this generalised theory, as displacement currents in conductors are negligible, is that of dielectrics.

In the first place, we may consider the propagation of V . We have

$$\begin{aligned}\nabla^2 V &= 4\pi \left(\frac{df}{dx} + \frac{dg}{dy} + \frac{dh}{dz} \right) \\ &= -K_1 \frac{d}{dt} \left(\frac{dF_1}{dx} + \frac{dG_1}{dy} + \frac{dH_1}{dz} \right) - K_1 \nabla^2 V.\end{aligned}$$

$$\begin{aligned}\text{Now} \quad \frac{dF}{dx} + \frac{dG}{dy} + \frac{dH}{dz} &= \int \frac{1}{r} \left(\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} \right) d\tau \\ &= - \int \frac{1}{r} \frac{d\rho}{dt} d\tau \\ &= - \frac{dV}{dt}.\end{aligned}$$

$$\text{Therefore} \quad \frac{1+K_1}{K_1} \nabla^2 V = \frac{d^2 V}{dt^2} - \frac{d}{dt} \nabla^2 \chi,$$

$$\text{and, finally,} \quad \frac{1+K_1}{K_1} \nabla^2 V = \frac{d^2 V}{dt^2} - \frac{1}{4\pi} \nabla^2 \int \nabla^2 \phi \frac{d^2 V}{dt^2} d\tau,$$

in which ϕ is a function of r .

This equation determines the mode of propagation of V . It represents wave-motion of a complicated character which may be analysed most easily by applying the equation to the case of a plane wave with the displacement at right angles to its front. There are two comparatively simple cases.

(i.) If $\nabla^2 \phi = 0$, i.e., $\phi = A + Br^{-1}$, the equation becomes

$$\frac{1+K_1}{K_1} \nabla^2 V = \frac{d^2 V}{dt^2} + B \frac{d^2}{dt^2} \nabla^2 V,$$

which represents wave propagation with velocity depending on the wave-length, and therefore involving dispersion.

For the plane wave

$$V \propto \exp i(mx - nt),$$

it leads to the condition

$$\frac{1 + K_1}{K_1} m^2 = n^2 - Bm^2 n^2,$$

and the velocity of propagation is

$$v = \left(\frac{1 + K_1}{K_1} \right)^{\frac{1}{2}} \left(1 - \frac{4\pi^2 B}{\lambda^2} \right)^{-\frac{1}{2}},$$

where λ is the wave-length.

The special case of $\phi(r)$ equal to zero is worth notice, as that would represent a theory in which the element of Neumann's integral, viz., $ids \ i'ds' \cos \epsilon/r$, is the mutual energy of two current elements. When the currents are not circuital, this leads to a condensational wave of the type here given.

(ii) If $\phi(r) = Cr$ (von Helmholtz's hypothesis) the equation becomes

$$\frac{1 + K_1}{K_1} \nabla^2 V = (1 + 2C) \frac{d^2 V}{dt^2},$$

denoting undulatory propagation with constant velocity

$$\left\{ \frac{1 + K_1}{(1 + 2C) K_1} \right\}^{\frac{1}{2}},$$

which agrees with von Helmholtz's result, when his notation $\frac{1}{2}(k-1)$ is written for C .

There is apparently nothing self-contradictory in the more general values of $\phi(r)$. The form $\phi(r) = Cr + A + Br^{-1}$, here considered, is notable for the case $C = 1$; as then the law of electrodynamic action between two changing charges would be simply that of the inverse square.

Next we shall consider the propagation of the electric displacement fgh . We have

$$\begin{aligned} \nabla^2 f &= \frac{K_1}{4\pi} \left(-\frac{d}{dt} \nabla^2 F - \frac{d^2 x}{dx dt} \nabla^2 \chi - \frac{d}{dx} \nabla^2 V \right) \\ &= K_1 \frac{d^2 f}{dt^2} - \frac{K_1}{4\pi} \nabla^2 \frac{d^2 \chi}{dx dt} + K_1 \frac{d\rho}{dx}, \end{aligned}$$

with two similar equations in g and h .

From these equations χ may be eliminated by means of the equations found for V

$$\frac{1+K_1}{K_1} \nabla^2 V = \frac{d^2 V}{dt^2} - \frac{d}{dt} \nabla^2 \chi,$$

$$\nabla^2 V = -4\pi\rho,$$

where

$$\rho = \frac{df}{dx} + \frac{dg}{dy} + \frac{dh}{dz}.$$

There result equations of the type

$$\left(\nabla^2 - K_1 \frac{d^2}{dt^2} \right) f = \frac{K_1}{4\pi} \frac{d}{dx} \left(\frac{1+K_1}{K_1} \nabla^2 V - \frac{d^2 V}{dt^2} \right) + K_1 \frac{d\rho}{dx}.$$

$$\text{Thus, finally,} \quad \left(\nabla^2 - K_1 \frac{d^2}{dt^2} \right) \left(\nabla^2 f + \frac{d\rho}{dx} \right) = 0,$$

$$\text{or} \quad \left(\nabla^2 - K_1 \frac{d^2}{dt^2} \right) \left\{ \frac{d}{dz} \left(\frac{df}{dz} - \frac{dh}{dx} \right) - \frac{d}{dy} \left(\frac{dg}{dx} - \frac{df}{dy} \right) \right\} = 0.$$

The three equations of this type are equivalent to only two independent equations.

They show that all displacements fgh for which the condensation ρ is zero are propagated with the constant velocity $K_1^{-1/2}$, whatever be the form assigned to $\phi(r)$. For, write

$$(f, g, h) = \left(\frac{dS}{dx} + f', \frac{dS}{dy} + g', \frac{dS}{dz} + h' \right),$$

so that

$$\frac{df'}{dx} + \frac{dg'}{dy} + \frac{dh'}{dz} = 0;$$

this is possible, for to determine S we have simply

$$-\rho = \nabla^2 S,$$

so that

$$S = V/4\pi.$$

These equations will then determine the mode of propagation of $f'g'h'$ subject to this condition of no condensation, because S disappears from them. The propagation of S or $V/4\pi$ has already been considered.

For a system of non-condensational waves of this kind, propagated along the axis of x , all the quantities must be functions of x ; therefore f must vanish; that is, the displacement must be perpendicular to the direction of propagation. These waves are therefore waves of transverse displacement.

We conclude that the propagation of waves of transverse displacement with this velocity K_1^{-1} is not a characteristic of any special theory, but forms a part of any conceivable theory which admits some sort of polarisation in the dielectric, and leads to the correct results for Ampère's case of circuital currents.

This cardinal result will still follow, even if χ is any function whatever. The degree of (mathematical) generality which this remark imparts may be expressed as follows. In a complete circuit the one thing essential to the established theory is that the electric force integrated round the circuit should be equal to the time rate of change of the magnetic induction through it, and, therefore, have an ascertainable value, though its distribution round the circuit is a subject of hypothesis. The conclusion that waves of transverse displacement will be propagated in a dielectric with velocity K_1^{-1} will hold good if we assume any form whatever for the electric force which does not violate this one relation, and also assume an electrostatic polarisation of the medium, equal at each point to the electric force multiplied by a constant $K_1/4\pi$. For the indeterminateness that may exist in the vector potential (or electric momentum) FGH is of the same type as that which may exist in the electric force PQR, and, therefore, as the equations show, may be merged in the latter. It would, perhaps, be difficult to conceive any more general hypothesis than this.

The increased generality which can be imparted to the theory merely leads to various modes of propagation of a condensational wave.

Comparison with Experimental Knowledge.

In the general theory of polarisation sketched at the beginning of this paper,

$$(f, g, h) = \kappa (P, Q, R);$$

therefore

$$K_1 = 4\pi\kappa.$$

The specific inductive capacity of the medium is

$$K_2 = \mu = 1 + 4\pi\kappa.$$

Thus

$$K_2 = 1 + K_1,$$

the units being here electrostatic.

Now, the results of various experimental investigations seem to place it beyond doubt that for dielectrics of simple chemical constitution the velocity of propagation varies as K_2^{-1} . Thus, in the recent experiments of Arons and Rubens,* the velocity of waves, 6 metres

* Wiedemann's 'Annalen,' vol. 42, 1891, p. 581.

long, guided by a pair of parallel wires, was measured by interference experiments when a part of the circuit was surrounded by various liquid dielectrics. The great length of the wave compared with the section of the conductor ensures that it travels with its front sensibly in the direction of propagation, and, therefore, that its velocity is normal; while the presence of the return wire limits its divergence into space. Their results are expressed in the following table which gives $K_2^{\frac{1}{2}}$, the index of refraction m for light waves of length 6.10^{-7} metres, and the index of refraction m' for the observed waves of about 6 metres long:—

	$K_2^{\frac{1}{2}}$	m	m'
Castor oil	2.16	1.48	2.05
Olive oil	1.75	1.47	1.71
Xylene	1.53	1.49	1.50
Petroleum	1.44	1.45	1.40

Thus the greatest deviation from correspondence for the longer waves is about 5 per cent. The correspondence of these numbers requires that the values of K_1 and K_2 should be sensibly equal for the substances tested, which can only be the case in the limiting form of the polarisation theory which constitutes Maxwell's displacement theory. In that case, as has been seen, the currents are all circuital; the Ampère-Neumann theory of electrodynamics suffices for all purposes, and there is no condensational wave.

The stand-point from which the theory of dielectric polarisation has been generalised in the theory here expounded is that of polar elements attracting according to the law of inverse squares in the manner of small magnets. In the results, however, this conception disappears and the phenomena are all expressed in the continuous manner by means of partial differential equations.

It is also possible, in Maxwell's manner, to ignore the attractions of the elements from the beginning, and simply to define the displacement as proportional to the electric force. The statical theory of condensers shows that in the dielectric the displacement must be circuital, for the characteristic equation of the potential must hold good. The displacement constant assumed by Maxwell is equal to the specific inductive capacity, in order to ensure that the charging current shall be continuous across the faces of a condenser. It might be proposed to take a less restricted form for this constant, with the result, of course, that the currents would be non-circuital. The investigation of this paper, however, proves that in all cases the velocity of the waves of transverse displacement is specified in terms of this displacement constant; and the experimental fact that in the simpler media it is determined in the same manner by the specific inductive capacity confines us to that value of the constant which is assumed

by Maxwell.* It is necessary to emphasise that it is of the very essence of a theory of this kind that the current in the dielectric is not circuital, and, therefore, that the electric volume density produced by the electric displacement varies with the time. This is so because the electrodynamic part of the electric force is not derived from a potential. Any investigation which restricts the current to be circuital is necessarily inconsistent with itself, except for the limiting case which forms Maxwell's theory.

A discrepancy of n per cent. (n a small number) between the observed velocity and K_2^{-1} would involve, by the formulæ at the beginning of this section, a difference of about $2n$ per cent. between K_2 and $K_2 - 1$, so that K_2 would be of numerical magnitude about $100/2n$; which determines the ratio in which the ordinary values of the inductive capacities of all media, including vacuum, would have to be multiplied, to make the polarisation theory not discordant with the observations.

The amount of discontinuity in the current at the surface of a conductor is the fraction K_2^{-1} of the total current across the surface. At the interface between two dielectric media, denoted by the values K_2 and K'_2 , the normal components of the displacement on the two sides are

$$(K_2 - 1)N/4\pi \text{ and } (K'_2 - 1)N'/4\pi,$$

where N , N' are the normal components of the electric force, so that

$$K_2 N = K'_2 N'.$$

Thus the discontinuity in the displacement is $(N' - N)/4\pi$ or $(K_2/K'_2 - 1)N/4\pi$ compared with a total displacement $(K_2 - 1)N/4\pi$; the ratio of these is $(K_2 - K'_2)/K'_2(K_2 - 1)$, which is less than the fraction K_2^{-1} , which corresponds to the surface of a conductor.

Thus, under the assumed circumstances, the ratio of the amplitudes of the condensational waves to those of the transverse waves would have a superior limit of the order $2n/100$; in the observations quoted this limit is at 5 per cent.

It is worth while to emphasise that if the polarisation theory were to take K_2 equal to unity for a vacuum, K_1 would be zero, and in a vacuum there would be nothing but action at a distance. It is thus an essential part of a theory like this that a vacuum has an absolute inductive capacity greater than unity, so that the ordinary value unity is merely a relative unit. Thus the transition to Maxwell's scheme, where the absolute coefficients are all assumed infinite, does not involve any undue stretch of the original hypothesis.

In the above, the relative velocities in different media of the

* Cf. J. J. Thomson, 'Brit. Assoc. Report,' 1885, p. 140.

transverse waves have been considered. The absolute velocity in a vacuum must take account of the fact that the ratio of the electrostatic and electromagnetic units of quantity has been altered by the factor K'_2 in the transition to Maxwell's theory, where K'_2 now represents the assumed absolute inductive capacity of the vacuum: thus the velocity for vacuum is $(1 - K'_2)^{-1}$ multiplied by the ratio of the electric units in vacuum, agreeing with von Helmholtz's result,* on writing this inductive capacity K'_2 for his constant $1 + 4\pi\epsilon_0$, and exceeding the velocity of light unless K'_2 is very great.

The theory of electrodynamics would thus appear to be, on all sides, limited to Maxwell's scheme, which has also so much to recommend it on the score of intrinsic simplicity.

The Society adjourned over the Whitsuntide Recess to Thursday, May 28.

Presents, May 14, 1891.

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* *Loc. cit.*, p. 627.

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Sir James Paget, Bart., F.R.S.

A series of Photographic Studies of the Normal Solar Spectrum.

Mr. George Higgs.

May 28, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Rev. A. M. Norman (elected June 5, 1890), was admitted into the Society.

The following Papers were read:—

- I. "On the Bases (Organic) in the Juice of Flesh. Part I."
By GEORGE STILLINGFLEET JOHNSON, M.R.C.S., F.C.S.,
F.I.C. Communicated by Professor G. JOHNSON, F.R.S.
Received April 28, 1891.

(Abstract.)

The author has endeavoured to ascertain by careful experiments how far the substances hitherto prepared from flesh are true "*educts*," and really present in the flesh itself, or merely products, due to (1) the action of chemical or physical agencies applied in the course of extraction, or (2) to bacterial action modifying the composition of the flesh before it comes into the hands of the operator.

Preliminary experiments are first described bearing chiefly upon the first-named source of error.

Liebig's process for extracting kreatine from the juice of flesh was modified by omitting the use of baryta-water, with the result that abundance of kreatine was obtained, mixed with acid potassium phosphate (KH_2PO_4). In Liebig's process potassium chloride is obtained after the kreatine has been separated.

A preliminary experiment is then described in which the author precipitated the albuminoid matters from the watery extract of fresh butcher's beef by means of solution of mercuric chloride, the filtrate depositing on standing a spherical precipitate, consisting of the mercury salt of the sarcous kreatinin, from which a tabular kreatinin was obtained isomorphous with the tabular kreatinin obtained by the author from human urine in 1887.

The special advantages of the method adopted by the author in isolating the kreatinin of urine are next detailed, after which a series of experiments are described in which muscle substance in different stages of freshness was extracted with water, the extracts treated by the mercuric chloride method, and the products compared.

Among these products is sarcous kreatinin, whose properties are fully described and carefully compared with those of urinary kreatinins previously investigated (*vide* 'Roy. Soc. Proc.,' vol. 43, pp. 493-534).

The final conclusion drawn is that sarcous kreatine is not present in fresh muscle, but results from bacterial action, whereas sarcous kreatinin is probably a true "educt."

II. "Note on Dr. Fenton Evans' Paper on the Pathogenic Fungus of Malaria." By W. T. THISELTON DYER, M.A., C.M.G., F.R.S. Received May 12, 1891.

The abstract of this paper published in this volume of the 'Proceedings' contains (p. 200) the following statement: "Alteration in the chemical composition of the nutrient medium . . . elicited the interesting fact that, under these circumstances, the organism can pass to a more highly developed state, displaying the structure and fructification of a highly organised fungus, but differing in certain important features from any fungus hitherto described."

This statement will remain on record, and can hardly fail to cause some perplexity to future students of the ætiology of malaria. I was present at the reading of the paper. The fungus exhibited was undoubtedly "highly organised." It was in point of fact a typical *Mucor*, and my friend Professor Marshall Ward, who was also present, was disposed to regard it as identical with the form known

as *Mucor racemosus*. The identification was so unmistakable that I utterly fail to understand in what "important features" the fungus differed "from any fungus hitherto described."

In the face of the undoubted fact that the fungus was a characteristic *Mucor*, it seems to me very improbable that it has a genetic relationship with any of the organisms found in the blood, and much more likely that its appearance in the nutrient medium was due to some experimental error.

III. "Method of indexing Finger-Marks." By FRANCIS GALTON, F.R.S. Received April 30, 1891.

Sufficient proof was adduced by me in a memoir read November 27, 1890, before the Royal Society ('Phil. Trans.,' B, 1891), of the extraordinary persistence of the papillary ridges on the inner surface of the hands throughout life. It was shown that the impression in ink upon paper of each finger tip, contained on the average from twenty-five to thirty distinct points of reference, every one of which, with the rarest exception, appeared to be absolutely persistent. Consequently that it was possible to affirm with practical certainty whether or no any two submitted impressions were made by the fingers of the same person.

In the present memoir I shall explain the way in which finger prints may be indexed and referred to after the fashion of a dictionary, and on the same general principle as that devised by A. Bertillon with respect to anthropometric measures, whose ingenious method is now in regular use on a very large scale in the criminal administration of France and elsewhere. I desire to show how vastly the practical efficiency of any such method as that of A. Bertillon admits of being increased by taking finger prints into account in the way about to be described.

It must not, however, be supposed that the use of indexing finger marks is limited to the above purpose, the power of doing so being equally needed for racial and hereditary inquiries. I do not dwell upon these applications now, simply because I am engaged in making them, and the results are not yet ready to be published. I ought, however, to mention that a great increase of experience has fully confirmed my earlier views, that finger marks are singularly appropriate subjects of anthropometric study owing to many distinct reasons. The impressions are easily to be made by anyone who has the proper appliances at hand. They are as durable as any other printed matter, and they occupy very little space. The patterns are usually sharp and clear, and their *minutiae* are independent of age and growth. They are necessarily trustworthy, and no reluctance is shown in per-

mitting them to be taken, which can be founded either upon personal vanity or upon an unwillingness to communicate undesirable family peculiarities.

Without caring to dwell on many of my earlier failures to index the finger prints in a satisfactory way, my description shall be confined to that which has proved to be a success. It is based on a small variety of conspicuous differences of pattern in each of many digits, and not upon the numerous minute peculiarities of a single digit. My conclusions are principally based on a study of the impressions of all ten digits of 289 different persons, but the tables about to be given refer only to the first 100 on my list. These are sufficiently numerous to serve as a fair sample of what we might always expect to find, while they are not too cumbrous to print and to discuss in full detail.

I described in my previous memoir the way in which the impressions had been made that were then shown. A plate of copper was blackened with printer's ink, the ink being of a rather fluid character, and spread very thinly and evenly over its surface by a printer's roller. The thumb, which was then the subject of discussion, was pressed and slightly rolled on the inked plate, and afterwards on the paper. In the present collection of all ten digits, four operations were used in each case. First the four fingers of one hand were simultaneously printed from, and then its thumb in the way above described; afterwards, the other hand was treated in the same way.

Though I have spoken and shall speak only of impressions, it is not really necessary for the purpose of compiling an index to make any impression at all. The entries that are wanted for the index can be derived directly from the fingers themselves.

I rely, for the purpose of indexing, on the three elementary divisions of primaries, whorls, and loops. They are severally expressed by the numerals 1 and 2, 3 and 4, 5 and 6. The reason of this double numeration is that most of the patterns have a definite axis. Those that are formed by ridges which proceed from only one side of the finger, will necessarily lie in a sloping direction across its axis pointing to the one side or the other according to that from which the supply of ridges proceeds. The only patterns that are symmetrically disposed about a vertical axis are *b* and to a lesser degree *a*, *c*, *h*, and *i* in fig. 1. Usually, and, as we may say, normally, the slope of the axis of the pattern is (roughly) parallel to a line drawn from a tip of the forefinger to the base of the little finger. All normal slopes, as well as all the patterns that have no definite axis, are expressed by the odd numerals 1, 3, or 5. All abnormal slopes are expressed by the even numerals 2, 4, or 6. It cannot be too strongly insisted that the words right and left are ambiguous and should not be used here.

FIG. 1.

<i>Elementary divisions</i>	<i>Index number</i>	<i>Symbols of Patterns</i>		<i>Index number</i>
		<i>symmetric.</i>	<i>sloped.</i>	
<i>Primary.</i>	1	  	   	1 or 2
<i>Whorls</i>	3	 	   	3 or 4
<i>Loops.</i>		<i>all sloped.</i>		
		     	   	5 or 6

The forefingers are the most variable of all the digits in respect to their patterns, their slopes being almost as frequently abnormal as not (see Table II); the third fingers rank next; the little finger ranks last, as its pattern is a loop in nine cases out of ten. I, therefore, found it convenient not to index the fingers in their natural order, but in the way that is shown at the head of the column of figures on the left side of fig. 2. There, the sequence of the numerals that express the

FIG. 2.

<i>L</i>	<i>R</i>	<i>L</i>	<i>R</i>	<i>Left</i>					<i>Right</i>					
123, 123	T4, T4	4	3	2	1	T	T	1	2	3	4	<i>Index</i>		
353, 333;	35, 35											38.2		
353, 333	35, 35											19.2		
333, 333	15, 35											5.2		
353, 653	35, 35											17.1		
353, 353	35, 35											16.1		
353, 453	35, 35											49.1		
363, 333	35, 35											3.2		
415, 555	35, 55											21.2		

patterns on the digits, is divided into two groups of three numerals and two groups of two numerals, as 355, 455, 55, 35. The first group 355 refers to the first, second, and third fingers of the left hand; the second group 455 to the first, second, and third fingers of the right hand; the third group 55 to the thumb and fourth finger of the left hand; the fourth group 35 to the thumb and fourth finger of the right hand. The index is arranged in the numerical sequence of these sets of numbers as shown in fig. 2 and in Table I.

Before translating the patterns into numerals, I find it an excellent plan to draw symbolic pictures of the several patterns in the order in which they appear in the impression, or in the fingers themselves, as the case may be, confining myself to the limited number of symbols shown in fig. 1, which have fairly well sufficed for my 289 sets or 2890 finger marks, as well as for many others. A little violence has of course to be used now and then, in fitting some unusual pattern to one of these symbols. But we are familiar with such processes in ordinary spelling, where the same letter does duty for different sounds, as *a* in the words *as*, *ask*, *ale*, and *all*. The merits of this process are many. It facilitates a leisurely revision of first determinations; it affords a pictorial record of the character of each pattern; it prevents mistakes between normal and abnormal slopes; it prevents confusion when changing the sequence of the entries from the order of the impressions to that used in the index; and, lastly, it affords considerable help to a yet further subdivision of the patterns. This may be inferred from the first two lines of fig. 2, which have the same index numbers, but whose pictured forms differ in respect to the two thumbs, and to the middle finger of the left hand.

I will now describe the symbols in detail, and show how such small difficulty as arises from rare transitional or border cases is minimised.

The primaries in their earliest and purest form are sufficiently expressed by the symbol *a*, fig. 1. From this elementary type all other sorts of patterns seem to be lineally descended. A fairly pure form of this type is seen in *b*; this is not infrequent in fingers, but I have not once met with it among some thousands of thumbs. A nascent whorl, still so immature as to count as a primary, is symbolised by *c*; similarly nascent loops, that should undoubtedly be counted as primaries, by *d* and *e*. When, however, the loop form is more pronounced and the pattern has been accepted as a primary only after reasonable hesitation as to whether it was not a loop, a dot is put inside the symbol, as in *f* and *g*, to serve as a warning. In this case, supposing another person to reckon the doubtful finger-mark as a loop and to refer and fail to find it under that head, he would make a second reference by treating it as a primary. A dot always means a possibly transitional case; thus *r* and *s* signify that they had been accepted as loops after some hesitation.

The whorls include circles, ellipses, and spirals, both simple and compound, whatever may be the direction or closeness of their twist. These are so apt to be confounded together unless the impression is from a *rolled* finger and is afterwards scrutinised and outlined (as explained in my previous memoir) that it seems best for the present purpose to group them all, with few exceptions, under the one symbol *h*. The exceptions are these. When two streams of ridges proceed from opposite sides of the finger and interlock, the symbol *i* is used, regardless of all other details. Again, when the whorl is crozier shaped, as in *j* and *k*, it is necessarily enclosed in a loop, but the loop is here ignored. If the crozier approaches very nearly and mistakably to either of the plain eyes *t*, *u*, it is dotted for a warning, as in *l* and *m*.

The loops in their simplest and common forms are shown by *n* and *o*. Frequently they have an internal offset which may be variously feathered or bent, short of being a whorl; all such cases are expressed by *p*, *q*. They have sometimes a conspicuous eye due to an internal curvature of the ridges upon themselves, or even to an eye in the central ridge; these are all expressed by *t* or *u*, in which the surrounding loop is left out in order to avoid multiplicity of lines. When the eye approaches nearly to a crozier as in *l*, *m*, the dotted symbols *r*, *w* are used.

In making a large and complete index, the symbols would, of course, be cast as movable types, and be printed with the letterpress. It will be seen from fig. 2 that there is space for 20 entries in one 8vo page.

I do not expect from my own reiterated experiences that there would be much trouble due to transitional cases, after a standard collection of doubtful forms had been established so as to ensure that different persons should abide by a common rule. I find much uniformity in my own judgment.

I give an index of 100 cases; they are the first that occurred in my catalogue of impressions, which are pasted in two rows on each page, and are consequently numbered 1, 1'; 2, 2', in order; but there are a few blanks, so the numbers in the index happen to run from 1 to 56', with some omissions, and not from 1 to 50'.

These cases afford data for roughly measuring the increase in power of discrimination obtained by basing indexes on the patterns of 1, 2, 3, 6, and 10 digits respectively. It appears from Table III that when all 10 digits are used, the number of different patterns observed in the 100 cases was 83; therefore the average number of references required to pick out a single well-defined case from among these 100 would be equal to 100 divided by 83, that is, to about $1\frac{1}{4}$.

It will also be seen from Table III that, owing to the large effect of correlation, an index based on all the ten digits is not much superior

in efficiency to one that is based on only six, namely, upon the first three fingers of both hands. In the 100 different sets there are 83 varieties of pattern in the one case and 65 in the other, which roughly accords with the relative efficiency of 5 to 4. When all the 289 cases are similarly treated, the relative efficiency comes out as 213 to 139, or roughly as 3 to 2. This is a little better but not much. It is, therefore, a fair question whether it is worth while to impress all the 10 digits. The chief advantage of doing so is to add to the volume of evidence, and to supply data which mutilation, or bad scars, or obliteration due to some exceptional cause might render of value. We also see from Table III that the three fingers of both hands are more than twice as efficient for the purposes of an index as those of one hand only; again, that three fingers are nearly twice as useful as two. I may mention that for my present inquiries into racial and hereditary patterns I am, for various reasons, dealing only with the three first fingers of the right hand, and slightly rolling the forefinger, so as to obtain a full impression of its pattern on the side of the thumb.

The greatest difficulty in constructing a uniformly efficient catalogue lies in the troublesome frequency of plain loops, so that even the method of picture writing fails to analyse satisfactorily the numerous 555, 555; 55, 55 cases. When searching through a large number of similarly indexed prints for a particular specimen, it is a very expeditious method to fix on any one well-marked characteristic of a minute kind, such as an island, or enclosure, or a couple of adjacent bifurcations, that may present itself in any one of the fingers, and in making the search to use a lens or lenses of low power, fixed at the end of an arm, and to confine the attention solely to looking for that one characteristic. The cards on which the finger marks have been made may then be passed successively under the lens with great rapidity. I fear that the method of counting ridges (as the number of ridges in the AH of my previous memoir) would be difficult to use by persons who were not experts. Anyhow, I have not yet been able to devise a plan for doing so that I can recommend.

Table I.—Numerical

Three first fingers.		Thumb and fourth finger.		Book I.	Three first fingers.		Thumb and fourth finger.		Book I.
Left, 1, 2, 3.	Right, 1, 2, 3.	Left th., 4.	Right, th., 4.		Left, 1, 2, 3.	Right, 1, 2, 3.	Left, th., 4.	Right, th., 4.	
111	111	15	15	page 52	215	115	55	55	page 48
"	"	"	"	20	"	255	55	55	20'
"	"	51	11	32					
"	"	55	35	37	253	155	55	55	7'
"	151	51	51	46					
					255	655	35	35	51
115	113	55	55	39					
"	115	15	15	55	333	155	55	35	14
"	"	55	55	4	"	333	35	33	2
"	155	15	55	34'	"	"	55	33	31'
"	"	55	55	25'	"	"	55	35	2'
					"	"	55	55	36
151	151	54	51	33'	"	352	33	33	45
					"	"	"	"	18
154	115	55	55	47	"	"	35	35	5'
					"	"	53	33	13
155	113	55	55	12	"	"	55	33	4'
"	115	55	55	20a	"	433	33	33	14'
"	116	35	53	1	"	555	35	55	55'
"	155	55	35	6	"	633	35	35	29
"	"	55	55	35'	"	"	"	"	13'
"	"	"	"	45'					
"	553	55	55	35	335	333	53	55	18'
"	555	35	35	23	"	653	55	55	30'
"	"	55	35	50'					
"	"	"	"	10	353	333	35	35	38'
"	"	55	35	54	"	"	"	"	19'
"	633	35	35	56'	"	353	15	55	6'
"	655	55	35	44'	"	653	35	35	17
156	553	35	35	7	355	353	55	35	16
					"	435	55	35	49

Index of 100 cases.

Three first fingers.		Thumb and fourth finger.		Book I.	Three first fingers.		Thumb and fourth finger.		Book I.
Left, 1, 2, 3.	Right, 1, 2, 3.	Left, th., 4.	Right, th., 4.		Left, 1, 2, 3.	Right, 1, 2, 3.	Left, th., 4.	Right, th., 4.	
365	355	55	55	page 3'	555	555	55	55	page 19
415	555	35	55	21a	"	"	"	"	3
433	433	35	35	10'	"	"	"	"	40'
453	355	55	55	32'	565	155	55	35	22
455	355	55	55	11	633	655	35	35	5
"	"	"	"	56	635	653	55	55	29'
"	455	35	35	41'	653	153	55	55	1'
515	153	55	55	23'	"	653	35	33	28'
"	156	55	35	49'	655	155	55	35	36'
553	153	15	15	37'	"	"	55	55	15'
"	333	55	35	13	"	"	"	"	12'
"	353	55	55	22'	"	335	55	55	21a'
"	553	55	35	27'	"	455	35	55	53'
"	"	"	"	16'	"	553	35	35	20a'
"	"	55	55	24	"	555	35	55	47'
555	115	55	55	40	"	"	55	55	44
"	151	55	35	27	"	"	"	"	52'
"	153	55	53	23	"	653	35	33	26'
"	253	35	35	26	"	"	35	55	21'
"	513	55	55	28	"	655	55	35	25
"	553	55	55	39'	"	"	55	55	51'
"	555	55	55	15	"	"	"	"	21
"	"	"	"	41	"	"	"	"	30
"	"	"	"	17'	665	655	55	55	46'

Table II.—Analysis of the 100 Cases in Table I.

Forefinger of left hand.		
Pattern.	Distinguishing number of pattern.	Number of cases.
Primary, plain.....	1	26
„ nascent loop, slope normal ... }	2	4
„ „ „ slope abnormal ... }		
Whorl, plain	3	23
„ with tail, slope normal..... }	4	6
„ „ „ slope abnormal..... }		
Loop, slope normal.....	5	21
„ slope abnormal	6	20
Total cases.....	..	100

Table III.—Further Analysis of the 100 Cases in Table I.

Number of times in which each pattern occurs.	Set of digits observed.							
	First 2 fingers of left hand.		First 3 fingers of left hand.		First 3 fingers of both hands.		All the digits of both hands.	
	Number of		Number of		Number of		Number of	
	Pat-terns.	Cases.	Pat-terns.	Cases.	Pat-terns.	Cases.	Pat-terns.	Cases.
1	5	5	13	13	49	49	71	71
2	4	8	5	10	6	12	10	20
3	—	—	1	3	4	12	1	3
4	1	4	1	4	4	16	—	—
5	—	—	2	10	1	5	—	—
6	1	6	1	6	1	6	1	6
—	—	—	—	—	—	—	—	—
10	1	10	—	—	—	—	—	—
11	—	—	—	—	—	—	—	—
12	—	—	1	12	—	—	—	—
13	—	—	1	13	—	—	—	—
14	—	—	1	14	—	—	—	—
15	—	—	1	15	—	—	—	—
16	2	32	—	—	—	—	—	—
17	1	17	—	—	—	—	—	—
18	1	18	—	—	—	—	—	—
Total cases	..	100	..	100	..	100	..	100
Number of different patterns }	16	..	27	..	65	..	83	

IV. "On the Anatomy and Physiology of *Protopterus annectens*."

By W. N. PARKER, Ph.D., F.Z.S., Professor of Biology in University College, Cardiff. Communicated by W. H. FLOWER, F.R.S. Received May 4, 1891.

(Abstract.)

The work which has resulted in the present paper was begun in Freiburg in the summer of 1888, when the author was fortunate enough, owing to the generosity of Professor Wiedersheim, to obtain a number of fresh specimens for examination. As so many interesting points presented themselves at an early stage in the research, a short preliminary notice, without illustrations, was published in the following autumn ('Berichte d. Naturforsch. Gesellschaft zu Freiburg i.Br.,' vol. 4, 1888).*

This notice merely forms the basis of the present paper, in which the whole subject has been worked out in greater detail. A number of new facts are recorded, some of the author's earlier conclusions modified, and the paper illustrated with 11 plates containing 71 figures.

With the exception of certain special details, the structure of the skeleton and of the nervous and muscular systems is not described, the paper consisting mainly of an account of other organs which have not received so much attention from previous observers, and of a comparison of *Protopterus* with the other genera of Dipnoi, so far as their structure is known, as well as with other Ichthyopsida.

The author returns his sincere thanks to the Council of the Royal Society for the grant out of which various expenses connected with the investigation were defrayed, as well as to Professor Wiedersheim, not only for the gift of abundant fresh and preserved material, but also for his continued help and advice during the progress of the work. To Professor Howes the author is indebted for many valuable suggestions.

A number of details with regard to the habits of *Protopterus* in captivity are given, and reference is made to Stuhlmann's observations with regard to its mode of life under natural conditions.

The paired extremities, the movements of which are more like those of limbs than of fins, show no connexion with the cheiropterygium, and, in spite of their considerable nerve supply, are evidently greatly degenerated structures as regards their free portions. Sensory organs are not present on them, and they therefore cannot have a tactile function. Their distal ends, like the apex of the tail, are very variable, and can undoubtedly be reproduced when lost by

* See also 'Nature,' vol. 39, p. 19.

accident. The tail is almost certainly not primarily diphyccercal, and shows signs of a possible origin from a heterocercal form.

The epidermis on the whole most nearly resembles that of Perenni-branchiate Amphibians, and gives rise to simple multicellular glands (which are most numerous on the snout), as well as to very numerous closely-packed goblet-cells, which produce the gluey secretion as well as the main substance of the capsule which surrounds the animal during the torpid state. The epidermis forms a regular and continuous layer over the derma, in which the cycloid imbricating scales are imbedded. Pigment cells are present in both layers of the integument, and the derma encloses nests of leucocytes here and there, small cells, apparently migratory leucocytes, being seen in places amongst the ordinary epidermic cells.

Integumentary sense organs, similar to those of Fishes and larval Amphibians, are present not only on the head and lateral line, but in various other regions of the trunk also; they are most numerous on the head. In young animals they are all superficial, and do not project below the general level of the epidermis, and this condition is retained in those situated on the trunk. On the head, the epidermis becomes involuted along certain lines to form grooves, which then become converted into sub-epidermic tubes, in which the sensory organs are situated, and which communicate with the exterior by an aperture at one end. The relations of the sensory organs of the trunk are therefore similar to those seen in young stages of Fishes and in Amphibian larvæ, while in the case of the head, they resemble those which are typical for adult Fishes. End-buds, similar in structure to the taste-buds of Fishes and Amphibians, are present on the tongue and oral epithelium, but are absent on the lips, and, as in Amphibians, do not occur on the surface of the body.

As regards its general structure, the olfactory organ most nearly resembles that of Elasmobranchs, but the presence of posterior nostrils raises it to a higher level. The position of the anterior nostrils beneath the upper lip is probably to be accounted for as an adaptation in connexion with the torpid state (*vide infra*). The space between the eyeball and its muscles and the orbit is filled with a delicate connective tissue; there are no orbital glands or eyelids. Four straight and two oblique muscles are present. The cornea is continuous with the derma on the one hand, and the sclerotic on the other; the latter is fibrous in young animals, and islands of cartilage first appear at the points of insertion of eye-muscles, and then gradually extend so as to chondrify the whole sclerotic. The eye resembles that of Amphibians; a *processus falciformis* and *campanula Halleri* are absent, and no ciliary muscles were observed, though possibly present; almost all the pigment of the eye is ectodermic.

No specialised glands are present in connexion with the greatly

folded epithelium of the oral cavity. The lips contain no muscles. The tongue, as well as the palate, is covered with blunt conical papillæ, on which the taste-buds are situated. Beneath the epithelium the tongue is composed anteriorly to the hyoid of a simple connective tissue, while posteriorly to the hyoid it is made up of extrinsic muscles, the main mass of which is continuous with the ventral musculature of the trunk. A horny cap is developed over each tooth from the overlying epithelium, which apparently becomes cut through by the sharp edges and points of the teeth, and which probably corresponds to the *cuticula dentis*. The thyroid is a small bilobed organ imbedded in the tongue just above the hyoid symphysis, and has the characteristic structure. The thymus consists of lymphoid tissue, and is situated dorsally and posterior to the branchial arches, surrounding the blood-vessels of the external gills.

The alimentary canal extends almost in a straight line from the mouth to the vent. A ventral, as well as a fenestrated dorsal, mesentery is present supporting the intestine. The so-called urinary bladder ("cloacal cæcum") opens into the cloaca dorsally to the intestine; the author compares it with the "processus digitiformis" of Elasmobranchs. A spleen and pancreas are present, imbedded in the thin walls of the stomach, and extending on to the proximal part of the intestine; they are covered externally by sparse muscular fibres as well as by the peritoneum. The relations of the pancreas therefore most nearly resemble those met with in Ganoids and certain Teleosteans. The pancreas is deeply pigmented, and its ducts open into the bile-duct. The pigmented walls of the intestine and the spiral valve are very thick, owing to the abundance of lymphoid tissue contained within them. With the exception of the bursa entiana, the internal walls of which are raised up into a number of deeply pigmented oblique folds, the whole of the mucous membrane of the stomach and intestine is perfectly smooth, and there is no indication of any differentiated gastric or intestinal glands.

Cilia are present on the epithelium throughout the stomach and intestine. The epithelium is columnar and stratified, and branched pigment cells extend into it in the greater part of the intestine. Small leucocytes can be recognised among the epithelial cells here and there. A layer of small-celled lymphoid tissue directly underlies the epithelium. In the spleen and lymphoid organs of the intestine two kinds of tissue are present: (1) a large-celled tissue, which forms the greater part of these organs, and which somewhat resembles embryonic connective tissue; and (2) a smaller-celled tissue, similar to that lying directly beneath the epithelium, and resembling that of ordinary lymphoid follicles. Large migratory cells are present in both kinds of tissue, many of which enclose

yellowish granules. Gradations between these and rounded cells of a deeper yellow or brown colour can apparently be made out; the latter are arranged in larger or smaller groups, and cells appearing to be intermediate forms between these and the ordinary black branched pigment cells can also be seen. The lymphoid tissue is penetrated by networks of blood-vessels, and it seems probable that the yellow granules mentioned above are due to the disintegration of red corpuscles, which are ingested by leucocytes, and then undergo some change, whereby the latter gradually pass into the condition of black pigment cells, which migrate through the epithelium, and are so got rid of. The muscular layers are very thin. A muscularis mucosæ is present, and the circular and longitudinal layers are represented, but the direction of the fibres is in many regions difficult to trace. Strands, only a few cells in thickness, extend throughout the lymphoid tissue of the intestine, and some of these unite to form a longitudinal band passing down the axis of the spiral valve.

An analysis of the contents of the gut, for which the author is indebted to Professor Baumann, while yielding negative results as regards the stomach, proves the presence of peptones, in small quantities, in the intestine. The question as to the mode of digestion and absorption of the food in *Protopterus* is discussed.

The branchial apparatus shows signs of considerable reduction. Internal gills are present on the posterior face of the hyoid, on both faces of the third and fourth branchial arches, and on the anterior face of the fifth. Three pairs of external gills were present in all specimens, even the largest, examined. The pulmonary apparatus, on the whole, more nearly resembles the air-bladder and its duct of certain Ganoids than the lungs and laryngo-tracheal chamber of Amphibians. The pulmonary branches of the vagus cross one another at the base of the lungs.

The blood is remarkable for the large size of its elements, which is only exceeded in the case of *Proteus* and *Siren*, as well as for the large proportion of white corpuscles in comparison with the red ones. Two forms of the former are described, in one of which fine radiating pseudopodia can be protruded, and different stages in the degeneration of the nucleus and cell-body could be observed. The chief points of interest with regard to the blood-vessels are as follows:—(1) the presence of a paired pulmonary artery, the left supplying the ventral, and the right the dorsal, aspect of the lungs; (2) the presence of a single true post-caval, along with a persistent left posterior cardinal vein; and (3) the single caudal vein, giving rise to a right and a left renal portal.

No external sexual differences could be observed, and amongst the specimens examined, females were the more abundant. The urino-genital organs are surrounded by masses of tissue resembling the

large-celled lymphoid tissue of the gut, but differing from the latter in becoming largely converted into adipose tissue. The kidneys probably represent the mesonephros, and their duct the Wolffian duct; nephrostomes are absent.

In unripe males, delicate Müllerian ducts are present. The sperm is conducted to the exterior by a duct, which is probably formed in connexion with the testis, quite independently of the excretory apparatus. The seminal tubules are directly connected with it, and it opens into the base of the Müllerian duct, the rest of which apparently aborts completely. Unlike most of the tissue elements, which are very large, and closely resemble those of the Amphibia, the spermatozoa are very minute, and are remarkable in possessing *two* vibratile flagella attached to the carrot-shaped "head." The generative organs of the female bear a striking resemblance to those of Amphibians. The oviduct corresponds to the Müllerian duct; the epithelium covering its internal folds shows signs of degeneration similar to those which have recently been described amongst Urodeles.

No traces of a sympathetic were found.

An account of the mode of life of *Protopterus* during the torpid period is given. The cocoon is provided with a "lid," perforated by a hollow funnel-shaped tube, which passes between the lips of the animal, and thus forms a passage for the respiratory current. The source of nutriment during the summer sleep lies in the adipose tissue in connexion with the gonads and kidneys and alongside the notochord in the tail, as well as in the lateral muscles, some of which, especially in the caudal region, undergo a granular degeneration. Very probably the latter is the precursor of the fatty degeneration, and, in all probability, leucocytes are the active transporting agents of the degenerated material. This assumption would help to explain the large development of lymphoid tissue in the body of the animal.

An analysis of the muscles, by Professor Baumann, shows that they do not retain quantities of the products of nitrogenous waste, as is the case in Elasmobranchs.

The systematic position of the Dipnoi is briefly discussed in the light of the new facts brought forward in the present paper. Although the Dipnoi present many points of resemblance to Fishes on the one hand, and to the lower Amphibians on the other, their connexion with any living forms of either class is probably a very distant one, and it is inadvisable to classify them amongst the Fishes. Owing to the absence of ontological evidence, and to the incompleteness of our knowledge of the palæontological history of the Dipnoi, it is impossible to construct a genealogical tree which will show, with any approach to accuracy, the probable connexion between them and other

Ichthyopsidan types. The most that can be said at present, with anything like certainty, is that the Dipnoi are the isolated survivors of an exceedingly ancient group, which was probably related to the ancestors of existing Fishes and Amphibians. Amongst the former, the connexion seems to be closest to the Elasmobranchs, more particularly to the Chimæroids on the one hand, and to such an ancient Selachian type as *Chlamydoselache* on the other; but at the same time, the Ganoids probably arose from the common ancestral stock not very far off. Though retaining many primitive characters, the Dipnoi, and more especially *Protopterus* and *Lepidosiren*, are in some respects highly specialised, the specialisation being largely due to a change of habit.

- V. "On the Constitution of the Terpenes, Camphors, and Camphor Acids." By J. NORMAN COLLIE, M.D. Communicated by Professor RAMSAY, F.R.S. Received April 29, 1891.

[Publication deferred.]

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OBITUARY NOTICES OF FELLOWS DECEASED.

IN ALEXANDER JOHN ELLIS the Society have lost an earnest and useful worker in several important branches of knowledge. He was a learned mathematician, an accomplished scholar, an original thinker, an indefatigable investigator, and an industrious writer; and his contributions to some of the more obscure and unfamiliar subjects of scientific study have been numerous, varied, and remarkable.

He was born at Hoxton in 1814, and his original name was Sharpe, having been changed to Ellis in 1825. It is believed that this was done in consequence of a bequest, made by a relative, to enable him to devote his life to study and research, unhampered by pecuniary cares; and if this was so, he certainly carried out most faithfully his part of the bargain.

He was placed first at Shrewsbury School, and then at Eton, after which he went to Cambridge, being elected a Scholar of Trinity College in 1835. Here he worked with zeal, bearing his specified career in mind, and in 1837 he came out Sixth Wrangler and First of the Second Class in Classics. He entered the Middle Temple as a student, and remained a member; but he appears to have had no serious intention of following the legal profession, and he was never called to the Bar.

The work of Mr. Ellis's life, as exhibited in his multifarious and voluminous writings, has been so extensive that it is impossible here to do it full justice. It must suffice to indicate some of the principal subjects to which his attention was given.

He first made himself known as a writer on Mathematics, having published, in 1843, a translation of Professor Martin Ohm's "*Geist der Mathematischen Analysis*." He afterwards continued to write, from time to time, papers on mathematical subjects, mostly published in the '*Proceedings of the Royal Society*.' Many of these were of a somewhat abstruse character, as "*On the Laws of Operation and the Systemization of Mathematics*," 1859; "*On Scalar and Clinant Algebraical Coordinate Geometry*," 1860, 1861, and 1863; "*On Stigmatics*," 1865 and 1866: while others were more practical, such as "*Problems in Hypsometry*," 1865, and "*On the calculation of Logarithms*," 1881. He also published, in 1874, a separate work consisting of five consecutive essays, and entitled "*Algebra identified with Geometry*."

The chief subject, however, of Mr. Ellis's labours was a branch of philology which he called "Phonetics," or the science of pronunciation. A few years after leaving Cambridge, he associated himself with Mr. Isaac Pitman in arranging a system of printing called phonotypy, which, by the aid of several new letters added to the Roman alphabet, gave the means of accurately representing the various sounds used in spoken language. In 1844, he published a description of this in a little work called "Phonetics; a familiar System of the Principles of that Science"; and this was followed by several other works, pointing out the disadvantages of the ordinary orthography, and advocating a general adoption of phonetic spelling. For many years he laboured industriously, and at considerable cost, to further this "Spelling Reform," and, as a means of exhibiting his views more completely, he undertook the transformation, into the new orthography, of many well known and standard works, such as the New Testament, the 'Pilgrim's Progress,' some of Shakespeare's plays, 'Paradise Lost,' 'Rasselas,' and so on. He also brought out on the same plan a monthly magazine called the 'Phonetic Journal,' and afterwards a weekly newspaper entitled the 'Phonetic News.' The latter first appeared on the 6th January, 1849, and it ran for three months, attracting much public attention from the strangeness of its typography, and the boldness of the aim embodied in its design.

The object aimed at was, however, far too gigantic to have a chance of success, and Mr. Ellis afterwards contented himself with using his system as a means of enabling him to describe and discuss pronunciation with greater accuracy of detail than previously, particularly as exemplified in the varieties of existing dialects. With this view, he modified it considerably in order to simplify the printing arrangements. In 1886, he produced what he called "Palæotype, or the Representation of Spoken Sounds by Ancient Types"; and in 1870, he laid before the Society of Arts an elaborate paper with a more popular educational aim, "On a Practical Method of Meeting the Spelling Difficulty in School and in Life." In this he proposed, not, as before, to abolish the ordinary spelling, but to use concurrently with it a phonetic orthography formed only of ordinary types, which he called "Glossic"; the objects were, as he put it:—

Too fasilitait lerning too reed.

Too maik lerning too spel unneseeri.

Too asimilait reeding and reiting too heering and speaking.

Too make dhi riseevd proanunsiaishen ov Ingglish aksesibl too aul reeders, proavinsheh and foren.

Mr. Ellis was not long in finding a worthy use for the phonetic instrument which he had thus elaborated. In the course of his work upon it, he had occasion to look into the history of Englis

pronunciation, and he was so fascinated by the novelty and extent of the subject that he determined to devote himself to its study; and it occupied him, more or less, all the remainder of his life. It resulted in a great work, of which the First Part appeared in 1869. It was entitled: 'On Early English Pronunciation, with special reference to Shakespeare and Chaucer, containing an Investigation of the Correspondence of Writing with Speech in England, from the Anglo-Saxon Period to the existing Received and Dialectal Forms,' &c., &c.

This part was, in a few years, followed by three others, but Part V required so much labour, that it was only finished in 1889. The whole work contains about 2500 pages, and was published jointly by the Philological, the Chaucer, and the Early English Text Societies. The Public Orator at Cambridge, speaking of it in 1890, said, very appropriately:—

"It may be confidently predicted that the day will come when all the varieties of our dialects, like the ancient languages of the Arcadians and the Cyprians, will have entirely faded out of human cognizance; and then most certainly there will daily accrue increasing honour to the works which this author has elaborated with such infinite labour."

Mr. Ellis was President of the Philological Society from 1872 to 1874, and from 1880 to 1882, and he wrote many other works and papers on this and other kindred matters.

Another subject in which Mr. Ellis took great interest was the scientific theory of music. During his phonetic investigations, he had wished to obtain an accurate physical explanation of the production of vowel sounds, and, on the suggestion of Professor Max Müller, he referred for this to the work published in 1863 by Professor Helmholtz, "*Die Lehre von den Tonempfindungen.*" In this, he found much more than he had sought for. He had studied music under Professor Donaldson, of Edinburgh (a physicist as well as a musician), and had acquired a desire to investigate for himself the physical basis of the musical art. He had found but poor satisfaction in the existing theoretical works, but the novel expositions of Helmholtz solved all his difficulties.

The first results of his study were three papers presented to the Royal Society in 1864, "On the Conditions, Extent, and Realization of a Perfect Musical Scale on Instruments with Fixed Tones," "On the Physical Constitution and Relations of Musical Chords," and "On the Temperament of Musical Instruments with Fixed Tones." But he had so high an opinion of the importance of Helmholtz's work that he undertook the laborious task of translating it into English, and the translation was published in 1875, under the title of "*The Sensations of Tone as a Physiological Basis for the Theory of Music.*"

Of Mr. Ellis, however, it may be said that *nihil tetigit quod non ornavit*, and, not content with translating excellently the 640 closely printed pages of the original, he ornamented it with a mass of elaborate theoretical matter of his own, amounting, in notes and appendices, to about 60 per cent. more. A second edition was published in 1885, to which large further additions were made.

He wrote other essays on musical subjects, among which were two important ones on the much-debated subject of musical pitch, read before the Society of Arts in 1877 and 1880, and rewarded by silver medals. The last paper he presented to the Royal Society, the end of 1884, was a joint one by himself and Mr. A. J. Hipkins, "On the Musical Scales of Various Nations," and this was followed, in March, 1885, by a larger one on the same topic at the Society of Arts. He further endeavoured to make his phonetic knowledge practically useful to the musical art by an excellent little work on Pronunciation for Singers.

Mr. Ellis was elected into the Royal Society on the 2nd June, 1864. He had previously made several communications to the Society, and he was afterwards a frequent contributor to its Proceedings. He served on the Council from 1880 to 1882.

He was elected a Fellow of the Society of Antiquaries in 1870, and in June, 1890, he was presented by the University of Cambridge with the honorary degree of Doctor of Letters.

It must be added that Mr. Ellis was highly esteemed by all who knew him or had transactions with him, for his amiability of character and his zealous, sincere, and unselfish devotion to the spread of knowledge. But few men have possessed his depth of acquirements, or his power of application, and still fewer have used them so well.

About six years ago, he issued a circular to his friends, intimating that the few years of his life he could reckon on would be fully occupied by the completion of his great work, and requesting them to disturb him as little as possible till it was done. The concluding volume appeared, as has been said, in 1889; and he immediately afterwards wrote an abridgement of it, which was published separately in 1890. He had had, however, a severe shock in the death of his wife; his health began to give way, and he died on the 28th of October, 1890.

W. P.

Professor JOHN MARSHALL, F.R.C.S., died on the morning of the 1st of January, 1891, at his residence, "Bellevue," Cheyne Walk, Chelsea, to which he had removed in the preceding summer from the house in Savile Row where he had spent the greater part of his professional life. He had for some years been in failing health,

suffering from winter cough and occasional attacks of gout. These conditions doubtless predisposed him to the acute attack of bronchitis which proved fatal after an illness of a few days' duration.

Professor Marshall was in his 73rd year at the time of his death, having been born on the 11th September, 1818, at Ely, where his father practised as a solicitor. After having received his education at a private school, John Marshall was apprenticed to a surgeon at Wisbeach, and in 1839 began his medical studies at University College and Hospital, then the largest and most popular school in England. Here he speedily attracted the favourable notice of his teachers—more particularly of Mr. Quain and of Dr. Sharpey. His acquaintance with Dr. Sharpey speedily ripened into a close intimacy and a warm friendship which continued unbroken till his death.

Mr. Marshall became a member of the Royal College of Surgeons in 1844. He now established himself as a practitioner in Camden Town, was appointed Assistant Demonstrator of Anatomy in University College, and subsequently made Curator of the Anatomical Museum. In 1848 he was appointed Assistant-Surgeon to the hospital, and in 1849 became a Fellow of the Royal College of Surgeons. For many years he taught practical and operative surgery at University College; but it was not until 1866 that he was appointed full Surgeon with charge of in-patients to the hospital. This important step was gained on Mr. Quain's retirement from the hospital, and in the same year, on Mr. Erichsen being transferred from the post of Professor of Surgery in the college to that of Holme Professor of Clinical Surgery in the hospital, Mr. Marshall succeeded him in the college professorship. Mr. Marshall held this appointment till 1885, when failing health and the pressure of other work compelled him to resign his office, both in the college and hospital. In recognition of his lengthened and distinguished services in these institutions, he was appointed Emeritus Professor of Surgery in the college, and Consulting Surgeon to the hospital.

Onerous and important as were his duties in University College and Hospital, these institutions were by no means the only scenes of Mr. Marshall's professional activity. For four years he was the Fullerian Professor of Physiology at the Royal Institution. He was early in life appointed Lecturer on Anatomy as applied to Art, first at Marlborough House, and then at the Government School of Art at South Kensington; and on the death of Mr. Partridge he was appointed to the honourable post of Professor of Anatomy at the Royal Academy. In 1873 he was elected a member of the Council of the Royal College of Surgeons, and in 1883 he became the President of that college. He was elected President of the Royal Medical and Chirurgical Society in 1883, and in 1887 became President of the General Medical Council—an office that he held at the time of his

death. During the latter years of his life Mr. Marshall took an active interest in the establishment of a teaching university for London, and was chairman of the association established for the promotion of that object. Mr. Marshall was strongly in favour of the establishment of a new teaching university for the metropolis, and of leaving the present University of London to discharge its duties as an Imperial examining body untrammelled by teaching functions.

Mr. Marshall was elected a Fellow of this Society in 1857. He twice served on its Council, viz., from 1868 to 1870, and again in 1880-81. He has contributed two papers to the 'Philosophical Transactions.' The first of these (1849), published in 1850 ('Phil. Trans.,' pp. 135-170), was "On the Development of the Great Anterior Veins in Man and Mammalia, including an account of certain Remnants of Fœtal Structure found in the Adult; a Comparative View of these Great Veins in different Mammalia, and an Analysis of their occasional Peculiarities in the Human Subject." This paper, which was the result of a laborious original research, formed a very important addition to embryology and to our knowledge of the morphology of the vascular system. The second paper, published in 1864 ('Phil. Trans.,' pp. 505-538), was "On the Brain of a Bush-woman, and on the Brains of two Idiots of European Descent." This paper was a most valuable contribution to our knowledge of the anatomy of the idiot brain. In it Marshall mapped out the convolutions, and in some measure may be said to have been a pioneer in that field of research which has yielded such important results to Ferrier, Horsley, and others of recent years.

The bent of Professor Marshall's mind was essentially and strongly scientific. Throughout his life he devoted much time, and paid close attention, to the study and the teaching of anatomy and physiology. As has already been stated, he was for a lengthened series of years Lecturer on Art Anatomy at the Government Schools of Art at Marlborough House and South Kensington; up to the time of his death he held the Professorship of Anatomy at the Royal Academy of Arts; and for four years he had been Fullerian Professor of Physiology at the Royal Institution. As a lecturer he was clear and precise; his hearers felt that he possessed a thorough mastery over the subject on which he discoursed, and the enthusiasm with which he treated his favourite studies could never fail to elicit a warm and sympathetic response in his class.

Marshall's published works were chiefly on anatomy and physiology. "The Human Body: its Structure and Functions" appeared in 1860, "Outlines of Physiology" in 1867, and "Anatomy for Artists" in 1878. On surgery he wrote but little. His most important contributions to it were a paper in the 'Medico-Chirurgical

Transactions' for 1851, on "The Employment of Electricity in Surgery," and the Bradshaw Lecture delivered at the Royal College of Surgeons in 1883, on "Nerve-stretching," and the Morton Lecture, on "Cancer."

Mr. Marshall took much interest in hospital construction, and strongly favoured the "circular-ward" system. Whatever may be the merits or demerits of that system—and on this point there is much discrepancy of opinion, Marshall had the satisfaction of seeing it introduced, mainly through his advocacy, into several hospitals in this country.

Few members of the medical profession can show so full a record of public and official work as could Professor Marshall, and by none has such work been done more thoroughly and more faithfully than by him. Yet he has been allowed to pass away with services such as these unrecognised and himself unrewarded by any public mark of dignity or of distinction.

J. E. E.



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Holland has produced more, perhaps, than its share of men whose names are likely to be held in lasting honour by mankind, and among them hardly one greater or nobler as a hero of science than FRANS CORNELIS DONDEBS.* In him rare gifts of nature were so happily blended, and turned to such good account for the advantage of his fellow men, as to make him an illustrious example of how much may be accomplished for our race in those quiet paths of life in which he was well content to pass his days. He was, indeed, doubly fortunate, for, while he bore a conspicuous part in the extension of knowledge and its beneficent applications, in fields which he found already ripening for the discoveries with which his fame will be ever associated, he lived long enough to see the rich results of his labours universally and gratefully acknowledged by his contemporaries.

He was born the 27th May, 1818, at Tilburg, a manufacturing town of North Brabant, in the Kingdom of the Netherlands, in a community almost exclusively Roman Catholic. His father was a simple burgher, kindly and studious, who, though he seems to have left the cares of business very much to his more practical wife, while he occupied himself apart with chemistry, music, and literature, was still full of active sympathy with the less studious life around him. Eight daughters had been born to them, but, as yet, no son, when the unexpected fulfilment of a long deferred hope induced, it was thought, a congestion of the brain, under which the poor father rather suddenly succumbed. The child was tenderly reared by the mother and elder sisters, in narrow circumstances, and was probably spoiled, for he became unruly,

* For a portrait see frontispiece to the present volume.

and had to be sent at seven to the village school of Duizel, in the vicinity. Here he rapidly acquired all that the humble master, Mr. Panken, could impart, and showed such precocity, especially in arithmetic, that the rustics would mount him on the table of the village inn, and give him sums to solve for half-pence. It was thus, perhaps, discovered that he might be safely entrusted with the payment of the weekly wages by an employer, who rewarded him by a little pocket money. "Imagine the little boy with the dark eyes peeping out of the black locks"—the fond mother would say—"sitting behind the desk to give the coins to the big workmen!" He was also made responsible for the steady going of the village clock. They used to call him "Master's Frans." In after years, when Donders, the great Professor, was secretly requiting, by substantial benefits to afflicted relatives, the love bestowed upon him in childhood, such trifling incidents as these were recalled and treasured up by loving hearts, and they are, therefore, deemed not unworthy of a passing record here.

As he grew to be eleven years old he became so useful in the school that his mother was asked to allow him to remain there as a tutor for two years more at a salary. Thus early did the clever lad begin to exercise that innate aptitude for teaching which he afterwards cultivated to such perfection. He was subsequently moved on to other seminaries at Tilburg and Boxmeer, learned easily to converse in Latin and French, and less fluently in Greek. English he acquired from schoolfellows, since become London merchants, and friends of after life. In music, too, he was an adept, taking the 2nd violin in quartetts.

His religious instruction he first received from a sister of charity (*béguine*), who prepared the children for the priest's teaching. His sister Thérèse seems to have been a remarkable woman. He would relate of her that she was chosen abbess over a pauper establishment by the bishop, although the youngest of the community, and therefore in her own eyes unworthy. A photograph of her shows her to have been very like Donders in features. It is not surprising, perhaps, that his early reveries were of the priesthood; and some interesting traits have been preserved, witnessing to his boyish fervour in this direction; but, with opening manhood, the current of his aspirations, from whatever cause, entirely changed, and he never afterwards for a moment regretted his resolve to embrace a medical career. Having this in view, he would have proceeded to Liège, where his eldest sister was settled, having married M. Grandmont, subsequently head of the eminent firm of publishers in that city. But the revolution was about to break out, which was to end in the severance of Belgium from Holland, so he turned aside to the University of Utrecht, entering it as a medical student at the age of seventeen, and becoming at the same time a pupil in the Military Hospital.

"Indescribable" he says, on the occasion of his jubilee, after a lapse of fifty years, "was the impression made upon me here by the chemical lectures and experiments of Nicholas de Fremery. When, for the first time, I mastered the notion that all that exists, in its infinite variety, is composed of a relatively small number of elements, which in certain proportions unite and reunite, it seemed to me as if, with the creation of the elements, the whole of nature had been given, and my imagination worked this out in its own way. Later on I became especially interested in Physiology, as taught by Schroeder van der Kolk."

The term prescribed for admission to his examination for his degree at Utrecht not having yet arrived, he anticipated it by at once proceeding to Leyden, where his unusual proficiency in Latin and his many accomplishments secured for him a brilliant reception from the academical body.* Thus accredited, he went immediately to Flushing as a military surgeon and health officer, and shortly afterwards was promoted to headquarters at the Hague. Here he worked intensely in the hospital wards, made autopsies, contributed papers to the medical journals, and was favourably noticed by the Director-General; who, being about to reorganise the Military Medical School at Utrecht, flatteringly invited him, then only in his twenty-fourth year, to give the courses on Anatomy, Histology, and Physiology. This was no light enterprise, for it included 18 lectures in the week for the 46 weeks which made up the scholastic year; but he undertook it joyfully, "feeling teaching to be his true vocation." Thus he came back already distinguished to his own University city, his home from that time onward. There he was soon to become famous.

G. J. Mulder, then recently appointed Professor of Chemistry in the University, was already powerfully contributing to give form to the new science of Physiological Chemistry, and his genius at once attracted and was attracted by that of Donders. The two soon became close friends and fellow workers, Donders occupying himself in every spare moment with microscopical researches in connexion with the chemistry of the elementary tissues, and publishing many original papers.† With Jac. Moleschott also, then very young, he established a lasting friendship, as well as with Opzoomer and others who, in their several ways, became eminent. In those days of opening manhood, Donders plunged eagerly and discursively into every avenue of spiritual and intellectual activity. Men of science, lawyers, divines, were alike his intimates, while, in general society, his

* His inaugural dissertation, based on original researches, was entitled "*Dissertatio inauguralis sistens observationes anatomico-pathologicas de centro nervoso*," 1840.

† *Vide, e.g., "Proofs of a General Physiological Chemistry," 1843-50, pp. 539 et seq.*

musical and artistic temperament, responsive to every refined emotion, his quick perception and ready memory, his geniality and conversational powers, made his handsome presence everywhere acceptable.* "Donders was then," writes Moleschott with fervid admiration, "a swelling rose-bud, whose calix leaves signified nothing but pure science, the flower leaves hidden glory. In one word, he was a man complete—perfect for his time of life." His bright intelligence, indeed, was able to assimilate without apparent effort all that it saw and read of in the active world around—a world then agitated by novel questions, of absorbing interest, regarding the Constitution of the Universe and the true import of Man's place and being in it.

In those days very recent advances in the methods and aims of exact research, as applied to various branches of science, had made it possible to penetrate more deeply than ever before into many of the profounder mysteries of nature, and some grand enlightenment seemed near at hand. During the years following 1840, one conception in particular, that of the *Conservation of Energy in Nature*, long foreshadowed, was rapidly assuming definite shape under the ordeal of exact experiment pursued on many converging lines. It could hardly, however, have been said to have become yet established, even in the minds of the most advanced physicists, ere Donders had clearly recognised its far-reaching importance in its special application to the Science of Life, the foundations of which his keen gaze was then freshly exploring. In the winter of 1844, when but twenty-six, in "only a lecture, not pretending," he modestly says, "to any high scientific worth," he casts a glance on the change of matter as the source of animal heat.† Here we already find him embracing in his view all nature, and looking confidently to her most general and all-pervading laws for the explanation of the enigma of life. "Animal heat is chemical heat;" but the final and irreversible proof of this, he shows in detail, "can only be given when science shall have proved that the *quantity* of heat in the animal body answers absolutely to the chemical change which takes place there." "All working in nature, all life on earth, rests on the change of the elements from which it is formed, but side by side with this change of matter stands a change of forces. Both are inseparably bound up together. As the change of matter is the condition without which no life exists, so the change of forces is the condition without which no life gives evidence of itself. An idea arises gradually in science, which finds confirmation everywhere, absolute contradiction nowhere, an idea

* His stature was 6 feet 1 inch; circumference of head, 24 inches, English.

† "A Glance on the Change of Matter of Epitellurian Life as the Source of Proper Heat of Plants and Animals," by Dr. Donders, Military Doctor, 2nd Class, at the Military School of Medicine, delivered in the Society of Sciences, Utrecht; Van der Post, Feb. 1845. [In Dutch, never translated.]

great and all-encompassing, fertile for the future development of science, it is the Permanence of Forces. No one molecule of matter can be destroyed, but neither can a minimum of Energy. Thus runs the important hypothesis which may come to be the soul of natural science. The forces change and join, they appear under different forms, but no force is annihilated. Determinate quantities of movement, heat, light, electricity, magnetism, and nervous force respond to each other, and can pass from one into the other." "There is therefore a sum of energy, just as much as there is a sum of matter; both are proportionate to each other, both remain always the same."

And Donders was hardly less prescient as he stood on the threshold of that other great achievement of our era, the doctrine of the *Evolution of Organisms on our planet*. The knowledge of the elements and of the elemental forces, then rapidly extending, was being more and more applied to the elucidation of certain vital problems, on which the greatest minds had long speculated in vain. Standing as we now do in the fuller light of those crowning disclosures of the progression of living nature through past ages which we owe chiefly to the genius of Darwin and of Wallace, dealing with an opulence of new materials for thought, it is very interesting to notice how Donders, in that nascent period, regarded this momentous subject. Already, in 1846,* he had briefly contested the then all but universally accepted teleological notion of the origination of organic forms by separate creative interpositions, accounting it to be arbitrary and unscientific; and soon after, on being called to the Professoriate of the University, he deemed the topic "weighty enough for a wider treatment, and because of its general bearing, well suited to an inaugural discourse."† Herein, after passing in review the grander features of the material universe and of the earth, as then known, he strives to show that the harmony everywhere pervading living nature, then usually explained by the principle of design (conformity to an end), is simply a necessary result of the conditions under which all organisms have come to be what they have been, or are. Though by no means denying the existence of a purpose in the phenomena of nature, he insists that a doctrine of the purpose can never become science, and can indeed only tend to obstruct the progress of science by lulling to sleep the spirit of enquiry into the laws governing the phenomena. These remain open to investigation in the field of life, just as in that of inanimate matter.

It is remarkable how firmly Donders here grasps the certainty that all life has been ever in process of being moulded into its specific forms by the continuous operation, through long ages, of laws

* *Vide* Gids, 1846, pp. 893 *et seq.*

† "The Harmony of Animal Life, a Manifestation of Laws." By F. C. Donders, 28th Jan., 1848. [Also in Dutch, never translated.]

implanted in matter and the forces of matter; and that these laws have gradually but necessarily at every stage been operative on the plastic organisation, adapting it continuously to the new conditions which it was ever encountering; in default of which adaptation and renovation of the disturbed harmony, the organisation itself could not have survived. The laws must be studied in the phenomena; and he particularly discusses and illustrates the operation of three laws, which for shortness' sake he calls those of *habit*, of *exercise*, of *inheritance*. But it is not enough, he adds, to deduce the necessity of the harmony from these laws; we must endeavour to fathom these laws themselves more deeply. The two former, those of habit and of exercise, mutually interacting, continually tend towards a restoration of the harmony between the organism as a whole and its surroundings, and between the several component parts of the same organism, as the harmony becomes by little and little disturbed in lapse of time by the intercurrent of altered conditions. The last law, that of inheritance, carries over into the future the accumulated modifications of the past, so far as they have survived in the latest offspring, thus preserving the continuity of life through successive generations, but only by essential changes in its forms. "Already some light dawns in science on the causes of the phenomena we referred to, the laws of habit and of exercise; and thus, ascending from cause to cause, without ever losing ourselves in dreams about the purpose, we approach, slowly it is true, but with firm step, the ideal point of view, according to which all the phenomena of nature will be seen proceeding from the attributes of the elements and elementary forces. And if once by an All-wise Omnipotence these elements and forces have been created for a predetermined purpose, and if the conditions of the whole future have been enshrined in their attributes, then also not a single drop of blood flows without purpose through our veins, but it is a purpose which lies outside the science of nature."

As Donders had originally approached physiology from the side of medicine, so now he had evidently come to meditate deeply on this great theme of the procession of organic forms down the tracts of time on independent grounds of his own, and rather as a physiologist than as a naturalist. As other matters were engrossing his attention, he did not pursue this one to further conclusions; but he welcomed with delight the publication, in 1859, of Charles Darwin's book, 'The Origin of Species;' became, subsequently, the friend and correspondent of its illustrious author; and visited him at Down. He also, some years later, undertook, at his request, more than one elaborate investigation,* designed to elucidate obscure questions relating chiefly to

* *E.g.*, "On the Action of the Eyelids in Determination of Blood from Expiratory Effort," by F. C. Donders, translated in Beale's 'Archives,' 1870. See also

his work, 'On Expression of the Emotions in Man and Animals,' then under preparation.

Almost casually, in one of his letters to Darwin (14th March, 1871), thanking him for a copy of 'The Descent of Man,' Donders thus summarises in our language his own views of 1847: "I always took a great interest in the question of the origin of organised beings. Even in 1847 I wrote and published an essay (*oratio inauguralis*) on the subject 'Harmony of Animal Life, a Manifestation of Laws,' containing, from the physiological *point de vue*, a further development of the doctrine which had been indicated by Lamarck, although the communications on this subject of Lamarck were as unknown to me as almost to every one, in that period. Fully excluding final causes from scientific research and theory, I tried to show how the infinite harmonical relations, on the one hand, between animals and surrounding nature, on the other hand, between the different parts and organs of every organism, are to be deduced from the laws of adaptation by habitude and by exercise, and from the laws of transmission. I applied the same on the psychical actions. I admitted the gradual evolution of the highest orders of plants and animals from more simple forms of spontaneous origin, and the origin of different species from the same source. I indicated the changes which are obtained by artificial selection, found the cause of continual progress in the circumstance that every not well adapted form necessarily is condemned to perish, but still was not aware of the influence of natural selection, your great and immortal discovery, the mighty factor, which alone allowed to give a full and special demonstration of the theory of descent. As I began to write, I had not the intention to mention to you my little book, but, telling about my special interest in the subject, I rather involuntarily inclined to explain it. And now, although it is written in Dutch, I could not resist my wish to send you a copy, in the hope that you will benevolently accept it. . . ." Darwin replies (18th March, 1871), ". . . I have been interested in what you tell me about your views, published in 1848, and I wish I could read your essay. It is clear to me that you were as near as possible in preceding me on the subject of Natural Selection." And afterwards (June 19th, 1871), ". . . When reading over your several letters, the thought has often crossed my mind how incomparably better an essay on expression you could have written than that which I shall be able to produce. . . ."; (April 8th, 1872) ". . . I feel, every day, that to write on Expression, a man ought to have ten times as much physiological knowledge as I possess. . . ."; and (December 21st, 1872) ". . . My book on Expression, in writing which I was so deeply indebted to your kindness"—

'Life and Letters of Charles Darwin,' by Francis Darwin, vol. 3, and C. Darwin 'On Expression, &c.,' 1872, p. 160.

sentences, honourable alike to each of these eminent men, exhibiting true modesty, transparent candour, and in all simplicity a most generous appreciation of merit in the other.*

But to resume the tenour of Donders' life. In 1847, that his services might the better be secured to his University, he was named Professor Extraordinary, there being no vacancy among the ordinary chairs; and such was the confidence inspired by his character that he was asked to select his own subjects for lecture. He chose four, viz., Forensic Medicine, Anthropology (especially for students in theology and law), General Biology, Ophthalmology. To this last he had been drawn, not only by its own intrinsic charm, but from his having, in the preceding year, in order to eke out his slender resources (for he had now wife† and child), undertaken a Dutch translation of the great German treatise of Ruete on that subject, and from having thereupon thrown himself, as was his wont, heart and soul into whatever lines of original research this work, as it proceeded, had suggested to him. It is impossible here to particularise, but several of these were among the more subtle problems lying on the borderland of physiological optics, problems remaining to perplex even the most observant practitioner, until by their solution the path is made clear to all alike. Suffice it that Donders in this way became more and more attracted towards Ophthalmic Practice; for with readiness he gave all the help in his power to the physicians and patients who were eagerly approaching him, as they heard of his discoveries in the physiology of vision. And when it was proposed to him by some of our passing countrymen that he should acquaint himself with English methods of treatment, then much esteemed abroad, he came to London in 1851, on the occasion of our first "Great Exhibition," returning by way of Paris. Some of the incidents of this journey he himself soon after placed on record,‡ and he always spoke of it as having had a great influence in moulding his life. It was his first travel, and it brought him, at least, one thing for which he had great reason to be thankful—the personal friendship of Albrecht von Graefe, an association soon to be fraught with splendid results for the expanding science of Ophthalmology; for these two men, both of the first capacity, laboured ever afterwards to advance it as brothers in council, and alike fruitfully; freely communicating their ideas to each other, always in perfect harmony of aim. While von Graefe, a stranger in London, was able to tell Donders of the European hospitals he had been visiting, and of the new clinical ideas he was

* *Vide* Chas. Darwin 'On Expression, &c.,' Nov. 1872, p. 160, &c. The writer is indebted to Mr. Francis Darwin for the opportunity of perusing these letters.

† His first wife was Ernestine J. A. Zimmerman, daughter of a Lutheran pastor. (She died Sept., 1887.)

‡ Notes on London and Paris, 'Nederlandsch Lancet,' 1852.

maturing, as well as of the construction in that year, by Helmholtz, at Königsberg, of a dioptric apparatus for rendering visible the fundus of the eye, Donders, a stranger there too, could, on his side, explain many discoveries of his own in the physiological field, and, among other things, declare the true nature of the act of accommodation, quite recently disclosed with certainty by his countryman Cramer, under, it may be added, his own inspiration and in his own laboratory. It was somewhat later, though independently, that Helmholtz arrived at the same conclusion.

It is not wonderful that Donders, on his return to Utrecht, should have already decided on adding to the abounding work of his four-fold lectureship, including the theoretical side of Ophthalmology, the onerous responsibility of its daily practice. He had, in fact, been gradually led to recognise more and more that this department of the healing art, from the very nature of its subject-matter, affords an ampler scope and a firmer ground than any other for the application and exemplification of those scientific principles which must eventually bear sway in all its departments, if vagueness and uncertainty are to disappear under the slow but certain advances of exact knowledge.* But in addition he was then swayed by a special impulse hard to be resisted. It had long been known that in animals having a *tapetum lucidum* the rays of light entering the eye through the pupil are in part reflected outwards by that shining surface along the lines of entrance; and in 1846 our countryman Cumming, too early lost to us, had shown that in man also such a reflex was in a certain way demonstrable. But, in 1851, Helmholtz discerned that it must surely be possible, by an optical contrivance, to render visible the reflecting fundus itself by bringing these emergent rays to a focus upon the retina of an observer; and, as just mentioned, such means he had devised. The *Ophthalmoscope* was thus given to mankind, a discovery rather than an invention, as Helmholtz has himself remarked—a revelation transforming ophthalmology, and of itself entitling that great man to our ever grateful remembrance. In the words of Donders, “the whole world spoke of it; every one wanted to see the ophthalmoscope, which revived long-lost hope.” But Donders felt that a sphere for its employment in Holland was still needed; and his fellow citizens, appealed to for this, and fired with some of his own enthusiasm, provided him with a temporary hospital; a few years later subscribing funds for a permanent one.† “That result,” he remarks, “was obtained through the influence of the discovery of the ophthalmoscope and the appearance of von Graefe at Berlin.” “In

* *Vide* Francisci Cornelli Donders, oratio de justa necessitudine scientiam inter et artem medicam, et de utriusque juribus et mutuis officiis, quam habuit die xxvi m. Martii a. MDCCCLIII, quum magistratum academicum deponeret.

† Opened February, 1859, with forty beds.

those days " (he proceeds) "—I may here tell what I have kept secret till now—I was invited by the medical faculty of Bonn to be the successor of Helmholtz [as professor of physiology]. It was the unanimous wish of all the members of the faculty, including Helmholtz himself, then about to leave Bonn. The offer might have been tempting. With a gift of 40,000 florins in my hand, for a purpose marked out by myself, it could not be thought of. The Ophthalmic Hospital thus founded was to be an institution for patients, but also for investigation and research in Ophthalmology in its widest range, in connexion with the University, by which both science and practice might be advanced; and not only did our students share its advantages, but foreign fellow practitioners made their appearance to witness our proceedings and to participate in our enquiries."

These last had reference to a variety of problems presented in the course of the practical work which Donders now entered upon, but chiefly to the "Refraction and Accommodation Anomalies," which were found to be greatly more common than had been supposed, and to admit in large measure of exact definition and correction. In 1858, there appeared the first of a long series of essays, in which, during six years, he was able to propound a complete doctrine, complete as it left his hands, both as to theory and practice, of the employment and prescription of corrective glasses, a subject never really mastered till then, and yet of the widest importance in everyday life, for the young, the middle-aged, and the old of all classes, and for all future time. His results, elaborated down to their minutest details, were then arranged and collected into a volume, which it was his wish to offer to the world first, in its entirety, in an English form, as a reminiscence perchance of the welcome he had experienced here in 1851. This volume, as translated from the Dutch MS. by Dr. Moore, of Dublin, and revised by himself, was accordingly published by the "New Sydenham Society" in 1864, and dedicated to an English friend.* It was soon out of print, passed into several languages, and must remain the permanent classic, both as to theory and practice, on the topics embraced by it. To attempt an analysis of it would be beyond the scope of the present notice. It constitutes the title on which its author takes rank above all his contemporaries as the main founder of a very large province of modern Ophthalmology.

But it must not be supposed that these results, memorable as they were, stood alone among the achievements of Donders in those fertile

* 'On the Anomalies of Accommodation and Refraction of the Eye, with a preliminary Essay on Physiological Dioptries.' By F. C. Donders, M.D., Professor of Physiology and Ophthalmology in the University of Utrecht. Translated from the author's MS. by Wm. Daniel Moore, M.D., Dublin. The New Sydenham Society, London, 1864.

years. He was also enriching Physiology in other directions, even though well-nigh exhausting his strength in doing so: for he had been also serving the University as Rector, an office which he relinquished in 1853.* In conjunction with Dr. Banduin he undertook a Manual of Physiology, which, however, he could only carry as far as the first volume, 'Special Physiology' (1853)—for its time a work of authority, and still the best record of contemporary teaching. In 1857, he discovered that in each vowel sound the mouth is tuned to a definite pitch, alike in men, women, and children using a common speech, and differing only with difference of dialect; this was confirmed by Helmholtz. Again, in 1865, Donders took the first step in a new field of research, by determining the rapidity of perceptions of Thought and of the Will. Others had arrived at the physiological time, or that required for reacting by a movement on a nervous irritation. But in the next succeeding years he carried these exquisite investigations much further, analysing the time taken in simple, and also in many and various complex, psychical processes, by a most ingenious and refined method, which he explained in 1876, at one of the Conferences at South Kensington in connexion with the Loan Collection of Scientific Apparatus.† Others of his physiological papers deserving special mention, among a great number, were: "On the timbre of the Vowels," "Muscular Work and Development of Heat in relation to the necessary Elements of Food," "On the Tongue-instruments in the Organs of the Voice and Speech," "Influence of the Vagus Nerve on the Cardiac Movements," "On Associations, congenital and acquired" (the latter are the result of habit in the individual, the former represent habit in the species), "The Chemical Phenomena of Respiration are a Process of Dissociation;"—indeed, to the close of his life he remained indefatigable in the domain of Physiology, almost continuously winning new laurels in one or other of its departments.

In 1862 an event had occurred having an important influence in this direction. By the death of Schroeder van der Kolk the chair of Physiology fell vacant, and it was immediately pressed upon his acceptance, with the understanding that a new Physiological Laboratory should be erected for his use. This was an appeal he could not withstand, though aware that it would involve a partial, and, gradually, an almost complete, relinquishment of his ophthalmic practice. But Physiology in its widest range, with the ample field it presented for Research, had been his first love, and to this his inclination now gradually led him back. His esteemed pupil, Snellen, became his colleague at the Hospital, and eventually succeeded him there; while Thomas W. Engelmann, who was to be in course of

* *Vide* note *, *ante*, p. xv.

† *Vide* 'Science Conferences,' 1876, Section Biology, pp. 224—228.

time his much loved son-in-law, became his assistant in the Laboratory, and finally, his eminent successor. Donders continued in these new circumstances to display the same marvellous productiveness as heretofore, and to animate, by his influence and example, the younger men attracted to him, often from distant countries. He delighted, as he had ever done, to make them taste the joy of becoming themselves the authors of some original work of value, and to engage their interest and help in his own laborious and systematic inquiries into whatever promised to benefit mankind in the sphere of the sciences he was cultivating.

No better example of this generous ardour of pursuit could be adduced than his method of dealing with the subjects of the Colour Sense and of Colour Blindness, then more and more attracting attention in relation to the public safety. It exhibits very aptly his many-sided excellence. While acquainting himself with the ideas of his predecessors, he first statistically ascertained, with accuracy for himself, the broader facts, engaging for this the aid of his younger colleagues and pupils. The delicate instruments which he was from time to time contriving in the course of his researches bearing on the theory of the Colour Sense and its defects, as on other subjects,* were constructed in a special department of his laboratory by the mechanician Kagenaar, whom he had reared from a youth and made his friend. His theoretical conclusions, as they were reached, were published in papers of permanent value.† Meanwhile he was also calling the attention of the higher officials of the railway and sea services, of his own and other countries, by all the means within his power, to the responsibilities they lay under for the lives of the community, in the matter of Colour Blindness; was framing rules for their acceptance, which they might suitably enforce; and pressing his conclusions to their final consequences in practical life, with such directness, moderation, and good sense, as compelled the attention and assent of administrators and statesmen. And that he might not fail in his immediate object of effecting a present benefit, he further, for many years personally undertook, without remuneration, the very considerable labour of superintending the carrying out of this matter on the railways of Holland, examining scrupulously into the more doubtful instances of supposed defect, so as to prevent injustice to individuals, and in every way facilitating the adoption of the new rules which he had suggested. No wonder that his countrymen,

* Many of these instruments were exhibited at South Kensington in the Loan Collection of Scientific Apparatus, 1876.

† His views of that day "On Colours and Colour Blindness" were well summarised by himself in the theatre of the Lucasian Professor at Cambridge, August, 1880, Sir George G. Stokes being present. See 'Brit. Med. Journ.,' 13th November, 1880.

even the humblest, followed him with grateful recognition when he appeared at the stations or moved among them.

Indeed, his life was one of incessant labour and benevolent endeavour to turn to useful ends each new insight into nature which he and others were acquiring. Sympathising with all, seeming to understand the sentiments and interests of all, he was generous as well as just in his judgment of others, yet ever courageous and firm in the assertion of whatever he deemed to be right and true. Indefatigable in the pursuit of truth, he was as able in imparting it. Eloquence, the graces of style, and the mastery of several languages combined to make him a great teacher. Even in his youth he had become conscious that to teach was to learn, and that to learn was the purest of intellectual enjoyments.

"I was already in correspondence with Donders," says von Helmholtz, in a letter to the present writer, "before 1856, when I lived in Königsberg. He had sent me his physiological treatise on Animal Heat and his handbook on Physiology, and as I had then made the first experiments on the change of form of the crystalline lens in accommodation, he told me about the somewhat earlier experiments made by Cramer in his laboratory. As far as I know, I first made his personal acquaintance during my stay at Bonn, between 1856 and 1859. He used to go in summer, with his then already ailing wife, to Cleve, to breathe purer air in that hilly country, as was then the fashion in Holland. From thence he came over to Bonn. I have also paid him a visit of a few days, in Utrecht, at that time, and lived in his house. . . . The loveableness, openness, and honesty of his character you know—I need not portray them to you. We have then and afterwards discoursed very much on scientific questions, as we many times and independently had taken the same problems in hand. He had, in Ophthalmology, the greater experience of patients, and I have learnt much from him in that respect; but even where it seemed to me that I must maintain my own opinion, I never observed in him the least sign of sensitiveness, or of too great warmth in defending his position. In his way of talking he had then already, as a young man, something of stateliness; he loved choice expressions, reminding one somewhat of the antique eloquence of the French Academy. But he was never prolix, indeed, rather concentrated, in his conversation, and I have always loved to listen to him, though in Germany we are very little accustomed to attend much to the artistic oratorical element in speaking. He was clearly a warm-hearted man with great ideal views, and he thought it his duty to give utterance to these ideal views before the world, and to show them in their height and significance. Moreover, he was aware of his capacity of impressing this with great force upon his auditory. Very beautiful in this respect was his last speech in handing over the Graefe Medal at

Heidelberg."* . . . "Our friendship has remained unclouded to the last."

Donders took, from time to time, a very conspicuous part in the assemblies and congresses of Science. Here he shone as a star of the first lustre. By universal consent he was a most admirable President, particularly where men of many nationalities were met together; for his wide and accurate knowledge and accomplishments, his tact, the grandeur of his simple earnestness, and his magnificent personal bearing acted as a spell on all. If the occasion seemed to demand it he would give an address, sentence by sentence, in two or more languages, with perfect mastery. He presided over the fourth International Congress of Ophthalmologists in London, 1872, and again, for the last time, over the seventh meeting of the same at Heidelberg, in 1888; over the sixth International Medical Congress, at Amsterdam, 1879; and he was Vice-President, as well as Royal Medallist, of the seventh International Medical Congress in London, 1881. In his own country, from 1865 onwards, he annually presided over the Physical Section of the Royal Academy of Sciences, and (alternately with the President of the Literary Section), was President *in pleno*.

But in 1883 he expressed the wish to withdraw from some of the more arduous of his many engagements. He was soon, alas! to cease from the service of the University for which he had done so much. By the law of the Netherlands a Professor must retire at seventy. He was approaching that age, apparently in full vigour, though indications of gouty congestions had more than once occasioned some disquietude to his friends. His Jubilee was celebrated at Utrecht, on the 27th and 28th May, 1888, amid the universal applause of his countrymen and of men of science everywhere. He was decorated by the King, and in his honour a commemorative medal was struck. Forty of his former pupils communicated each an original scientific paper to a memorial Festival volume, published by a committee.† The Royal Society of London, which had elected him a Foreign Member in 1866, asked three of its Fellows, Sir Joseph Lister, Mr. Jonathan Hutchinson, and Dr. Hughlings Jackson, to convey to him its congratulations; our Physiological and Ophthalmological Societies also sent deputations. Indeed, hardly one civilised country but was in some way represented. A large sum had been subscribed, and was placed at his disposal. He assigned it to the establishment of Travelling Fellowships in Physiology and Ophthalmology, to be attached to the Universities of the Netherlands, with Utrecht as a centre; not without a glance, perchance, at an early incident in his own fortunate career. That career he now passed in review

* Viz., to von Helmholtz himself, 9th August, 1886. *Vide* 'Festsitzung der Ophthalm. Gesellschaft, &c.,' Rostock, 1886.

† 'Feestbundel Donders-Jubiläum, &c.,' pp. 546, Plates xvii.

before his assembled friends and pupils in a speech of touching simplicity and eloquence—a lasting and truthful survey of the lifework he had been enabled to accomplish. “I may be thankful,” he concluded, “for the life granted me. I stand here comparatively strong, and also ready to do what may be given me to do. I stand here, having reviewed my life, and having seen how manifold were the advantages that have been allotted to me. All this makes me grateful, fervently grateful, especially to the Eternal Source of all that exists, of which it is not given to man—a speck in the infinite space, a breath in the infinite time—to form an idea; he can do no more than bow reverently in absolute submission. This submission has been asked of me more than once. I hope also to find the strength to submit to what may be required of me in the future.”

Some sorrowful bereavements had indeed befallen him: in the loss, in 1870, of his only child Marie, after giving birth to twins; and more recently, in 1886, of the beloved and admirable partner of many years, after a long and distressing illness, through which he had nursed her with the tenderest assiduity. But he remained steadfast and full of trust, and he had many compensations. The retrospect of his life was happy; his contemporaries loved and honoured him as few men have been loved and honoured, recognising in him in a rare degree the possession and harmonious, fruitful, and lifelong exercise of some of the greatest and best attributes that can adorn human nature.

The last illness of Donders was sudden, as his father's had been, and in him, too, it was the circulation of the brain that failed. That powerful organ of sweetest feeling, high aspiration, and self-restrained will, which had enabled him to accomplish so much, even measuring for us the velocity of thought, was now itself to give way. He had come on a visit to England in October, 1888, and seemed to be supremely happy in the renewal of his domestic life. Most interesting was it to listen to the themes he opened as to the work he might soon undertake in the studio of his refined home, tracing the springs of Art to their most secret source in the very constitution of man's bodily organisation, subject in all respects to the conditions and limitations imposed by physiological laws. The operations of these laws he had long delighted to track out to their remotest consequences, and to communicate his conclusions to masters in art of the quality of his friends Sir Fred. W. Burton and G. F. Watts. His studies had always inclined him in this direction, and he now hoped to execute a design he had long cherished, of illustrating his conceptions and views by reference to the life of Leonardo da Vinci, the great artist with a scientific turn of mind, to whose figure he had ever felt himself especially attracted. In such a field, had his days

been prolonged, it is probable that he would still have left behind him rich legacies of thought (for who so capable?), in which rigidly exact definitions of scientific truth would have been seen to be in truest harmony with the most exquisite sensibility to every form of beauty, natural or artistic.

But the joy of anticipation was a perilous joy, and could not be long supported. Pathetic were the circumstances of the last fatal seizure, for in the intervals of illness he knew and weighed as in a balance of the laboratory all its phases, with unimpassioned serenity and resignation, though not without sadness, while witnessing the grief of those who loved him, and recalling the scenes in which he had acted so conspicuous a part. He rallied sufficiently to be able to return home under the escort of his brother-in-law, who had been summoned to his bedside in England. But relapses recurred, with varying alternations and pauses. During these weeks of suspense, so agonizing to his friends, he often spoke of the insoluble riddle of existence, and of the hope of a future reunion. One afternoon he had walked with assistance in front of his house (it was his last walk), and he seemed refreshed. "We have had a nice walk, and you are better," said one. "Yes," he replied, "a beautiful walk—is it not a beautiful walk?—to *Eternity*!" He died on the 24th March, 1889, within a year of his jubilee.

The day of his burial was indeed one of gloom in Utrecht.

The second wife of Donders was Bramine, daughter of Mr. P. F. Hubrecht, Secretary of the Home Office at the Hague, sister of Professor Hubrecht, of Utrecht, a lady of noble disposition and of wide culture. We owe to her remarkable talent several fine portraits of him, for one of which, a three-quarter length, painted for his jubilee, she received the award of a gold medal at Munich in 1888. It is destined for the National Museum at Amsterdam. Others are, a half-length, with his decorations, in the Hall of the Professors at the University; one at the Ophthalmic Hospital, representing him as in 1864, soon after the foundation of the hospital; and a fourth, as seated in his study, with the bust of von Helmholtz at his side, in the last year of his life, painted for his twin grandchildren, Frans and Paula Engelmann. There are also two life-size heads by Mr. Watts, R.A., painted in 1873–75, during some of his brief visits to England. One of these, never completed, but remaining a grand sketch, forms one of Mr. Fred. Hollyer's series of Mr. Watts's portraits, and has been reproduced as the frontispiece to the present volume of the 'Proceedings of the Royal Society' by Mr. Dew-Smith, of the Cambridge Engraving Company. Both this and the jubilee portrait were exhibited at the Royal Society Conversazione,

18th June, 1890. There is, lastly, a life-size oil picture of Donders as he was in 1881, for which the writer is indebted to the kindness of his old friend E. U. Eddis, Esq.

Some Published Notices of FRANS CORNELIS DONDERS.

Some Earlier Ones.—(1.) A. Kölliker, Skizze einer wissenschaftlichen Reise nach Holland und England in Briefen an C. Th. v. Siebold. Zeitschrift f. Wissensch. Zoologie v. C. Th. v. Siebold und Kölliker, vol. 3, 1850, p. 86. (2.) F. C. Donders, Notes on London and Paris. Nederlandsch Lancet, 1852 (translated into English at the time). (3.) Photographs of Eminent Men of all Countries, with Brief Analytical Notices of their Works. By T. Herbert Barker, M.D., and Ernest Edwards, M.A., 4to, London, 1867-8, vol. 3, pp. 93-104. *Later.*—(4.) F. C. Donders, Discours d'Ouverture, Congrès International des Sciences Médicales, Amsterdam, 1879. Edition corrigée. (5.) Festsitzung der Ophthalmologischen Gesellschaft in der Aula der Heidelberger Universität am 9 Aug., 1886.—Ueberreichung der Graefe-Medaille an Hermann von Helmholtz, Rostock, 1886. (6.) Het Jubileum van Professor F. C. Donders gevierd te Utrecht op 27 en 28 Mei, 1888.—Gedenkboek uitgegeven door de Commissie, Utrecht, P. W. Van de Weijer, 8vo, pp. 210, 1889. (7.) Franciscus Cornelis Donders. Festgruss zum 27 Mai, 1888. Dargeboten von Jac. Moleschott, 8vo, pp. 51, Giessen, 1888. *Some Obituary and other Notices.*—(8.) Mort de Donders. Annales d'Oculistique, publiées par le Dr. Warlomont, 8vo, pp. 141-144, Mars-Avril, 1889. (9.) Franz Cornelius Donders, M.D. Brit. Med. Journ., 30th March, 1889 (by W. A. Brailey). (10.) F. C. Donders, par le Dr. E. Landolt, Extrait des Archives d'Ophthalmologie, Mai-Juin, 1889 (a just and eloquent tribute, translated in 'The Illustrated Medical News,' 14th September, 1889, with a portrait). (11.) 'Die Ophthalmologische Gesellschaft während der ersten fünf und zwanzig Jahre ihres Bestehens, von 1863 bis 1888. Im Auftrage des Ausschusses zusammengestellt und herausgegeben von Wilhelm von Zehender, 8vo, pp. 111, Rostock, 1888. (12.) Commemorazione dell' Accademico onorario Francesco Cornelio Donders, &c. Letta dal Prof. G. Colasanti nella seduta della R. Accademia Medica di Roma il 28 Aprile, 1889, 8vo, pp. 16. (13.) F. C. Donders, Klinische Monatsblätter für Augenheilkunde, herausgegeben von Dr. von Zehender, Mai, 1889, 8vo, pp. 163-168. (14.) Prof. Snellen (notice of Donders) 'Het Nederlandsch Gasthuis voor Behoeftige en Minvermogene Ooglijders gevestigd te Utrecht,' 2^o Juli, 1889. (15.) F. C. Donders et son Œuvre, par Prof. J. P. Nuel (Liège), 'Ann. d'Oculistique,' 8vo, pp. 1-107, 1889 (with an analysis of 208 of Donders' papers and treatises, and a portrait—a full and admirable

account). (16.) F. C. Donders, Gedenkrede gehalten in der feierlichen Jahressitzung der Budapester Kön. Gesellschaft der Aertze am 14 Oct., 1889, von Dr. W. Goldzieher, 8vo, pp. 28. (17.) Bericht über die Zwanzigste Versammlung der Ophthalmologischen Gesellschaft, Heidelberg, 1889; redigirt durch W. Hess und W. Zehender, Rostock, 14 Dec., 1889. (18.) Mannen van Beteekenis in onze Dagen, Redactie: Dr. E. D. Pijzel.—Prof. Donders, door Dr. B. J. Stokvis, Haarlem, 1889. (19.) F. C. Donders,—von Horstmann, 'Deuts. Med. Wochenschrift,' 1889, No. 14. (20.) F. C. Donders, by Wenckebach, in 'Students' Almanach' of Utrecht, Jan., 1890. (21.) Franciscus Cornelius Donders, by Henry Williams, M.D., Prof. of Ophthalmology in Harvard University, in Proc. Amer. Acad. Arts and Sciences, vol. 24, pp. 465-470. (22.) Franciscus Cornelis Donders, in 'Onderzoekingen gedaan in het Physiologisch Laboratorium der Utrechtsche Hoogeschool.' Uitgegeven door Th. W. Engelmann en C. A. Pekelharing. —Veerde Reeks, I. 1., Utrecht, C. H. E. Breijer, 1890 (a true and deeply interesting tribute to Donders' work and character, by his son-in-law, signed "E.").

W. B.

Jolduynds, Dorking, 24th March, 1891.

JOHN CASEY was born at Kilkenny, co. Cork, in May, 1820, and died 3rd. January, 1891, in Dublin. He was educated at first in a small school in his native village, and afterwards in a larger school in Mitchelstown. He became a teacher under the Board of National Education in various schools, including Tipperary National School, and ultimately Head Master of the Central Model Schools, Kilkenny. He then began to devote himself to mathematics. Being asked to solve "Poncelet's Theorem," he solved it geometrically, having, in doing so, discovered for himself much of the science of modern geometry. In connexion with this he began a lifelong correspondence with Dr. Salmon and the late Professor Townsend, at whose suggestion he entered Trinity College, Dublin, in 1858, and obtained Sizarship in 1859, and Scholarship in 1861, and his B.A. degree in 1862.

From 1862 to 1873 he was Mathematical Master in Kingstown School, which during that time obtained a reputation for training successfully for the Indian Civil Service Examinations. From 1873 to 1881 he was Professor of Higher Mathematics and Mathematical Physics in the Catholic University, and from 1881 until his death was a Fellow of the Royal University, and Lecturer in Mathematics in University College, Stephen's Green. In 1866 he was elected a member of the Royal Irish Academy, and a member of its Council in 1872, and was a Vice-President thereof for five years. In 1869 the University of Dublin conferred on him the degree of LL.D. Hon. Caus. He was

elected a member of the London Mathematical Society in 1874, and a Fellow of the Royal Society in 1875, member of the Sc. Soc. of Brussels in 1878, Corr. Memb. of the R. Soc. of Sciences of Liège in 1887, member of the Soc. Math. de France, 1884, and LL.D. Hon. Caus. R.U.I. in 1885. In 1873 Trinity College, Dublin, offered him a Professorship of Mathematics, but he reluctantly preferred to further the advancement of Catholic education by working for the Catholic University. In 1878 he was a Secretary of Section A of the British Association meeting in Dublin, and in the same year the R.I. Academy conferred on him a Cunningham Gold Medal.

In 1881 he commenced a series of mathematical class books for University and college students, which have acquired a deservedly high reputation, and some of which have been translated. From 1862 to 1868 he was one of the editors of the 'Oxford, Cambridge, and Dublin Messenger of Mathematics,' and for several years was Dublin correspondent for the 'Jahrbuch über die Fortschritte der Mathematik.' The Norwegian Government presented him, in 1881, with the works of Abel. He carried on an extensive correspondence with most of the leading European mathematicians, all of whom held Casey's work in high esteem.

His work was almost wholly confined to plane geometry, in which his papers have earned for him an established reputation. Professor Cremona has well described them as exhibiting the great elegance and ability with which he treated the most difficult and interesting questions. John Casey will ever be remembered as one of the very small band of eminent mathematicians who, self-taught, raised themselves from the grade of elementary teacher to University Professor.

In the Royal Society's Catalogue of Scientific Papers (1800--1883) there are eighteen titles under Professor Casey's name, their dates ranging from 1861 to 1880.

G. F. F. G.

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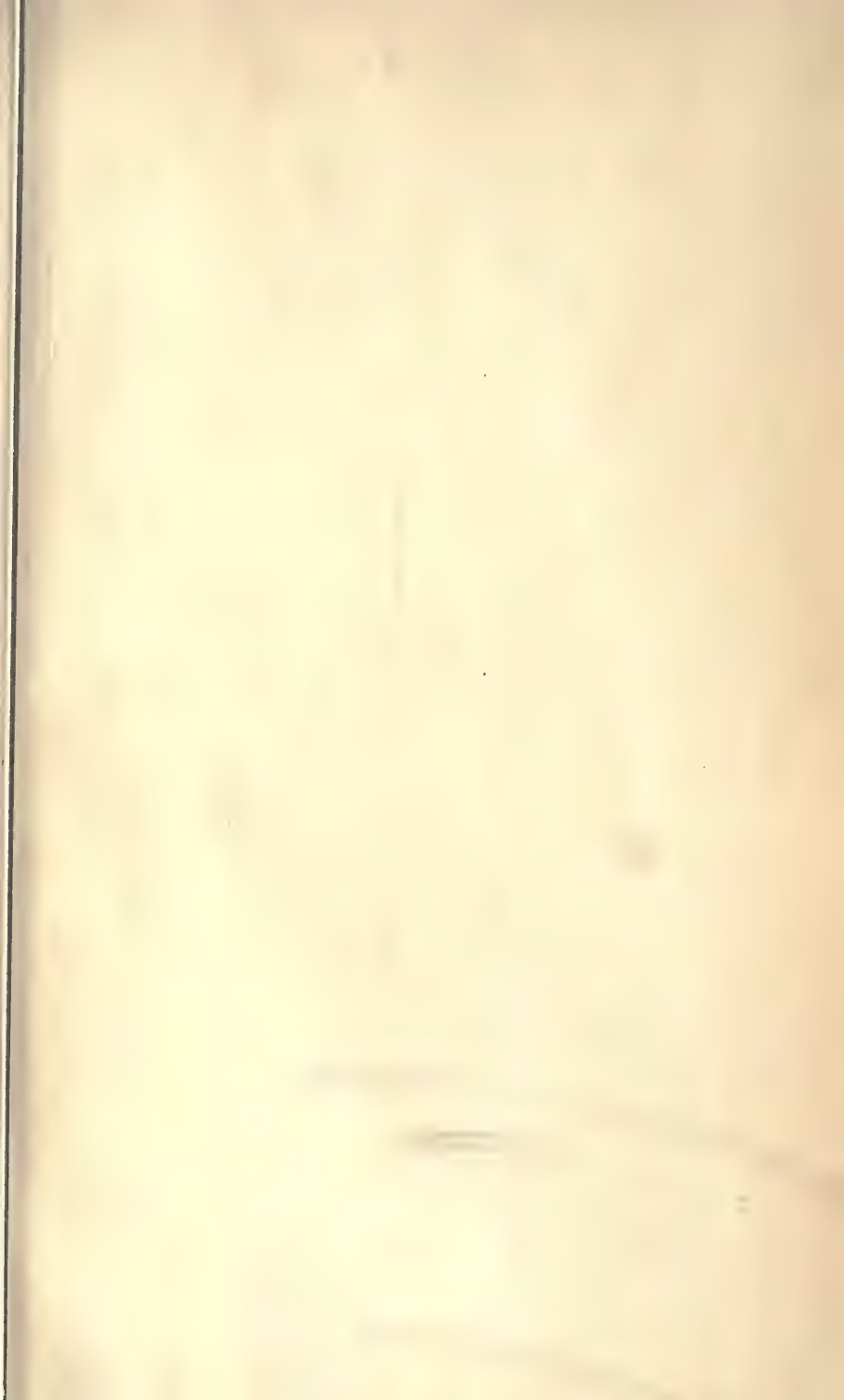
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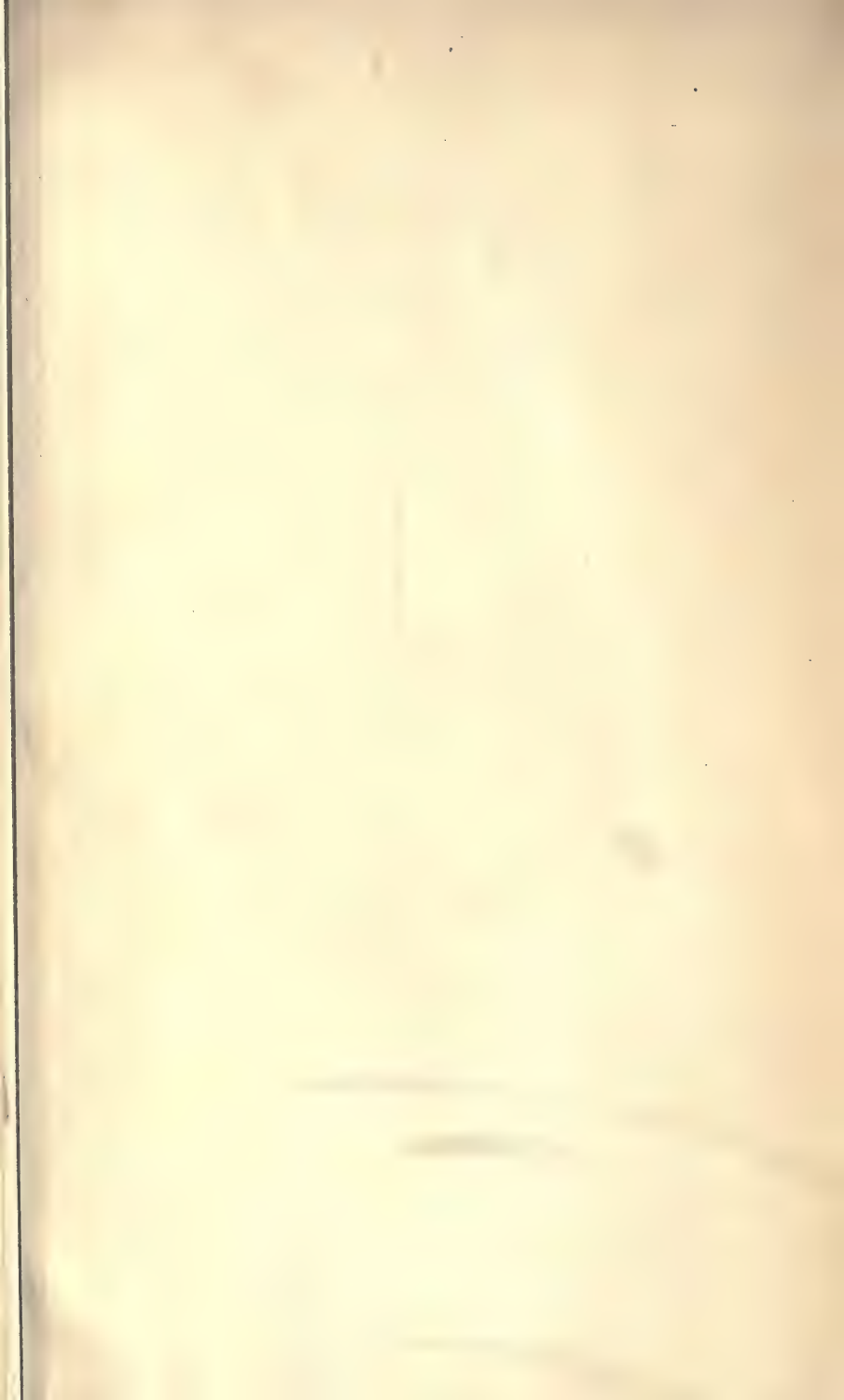
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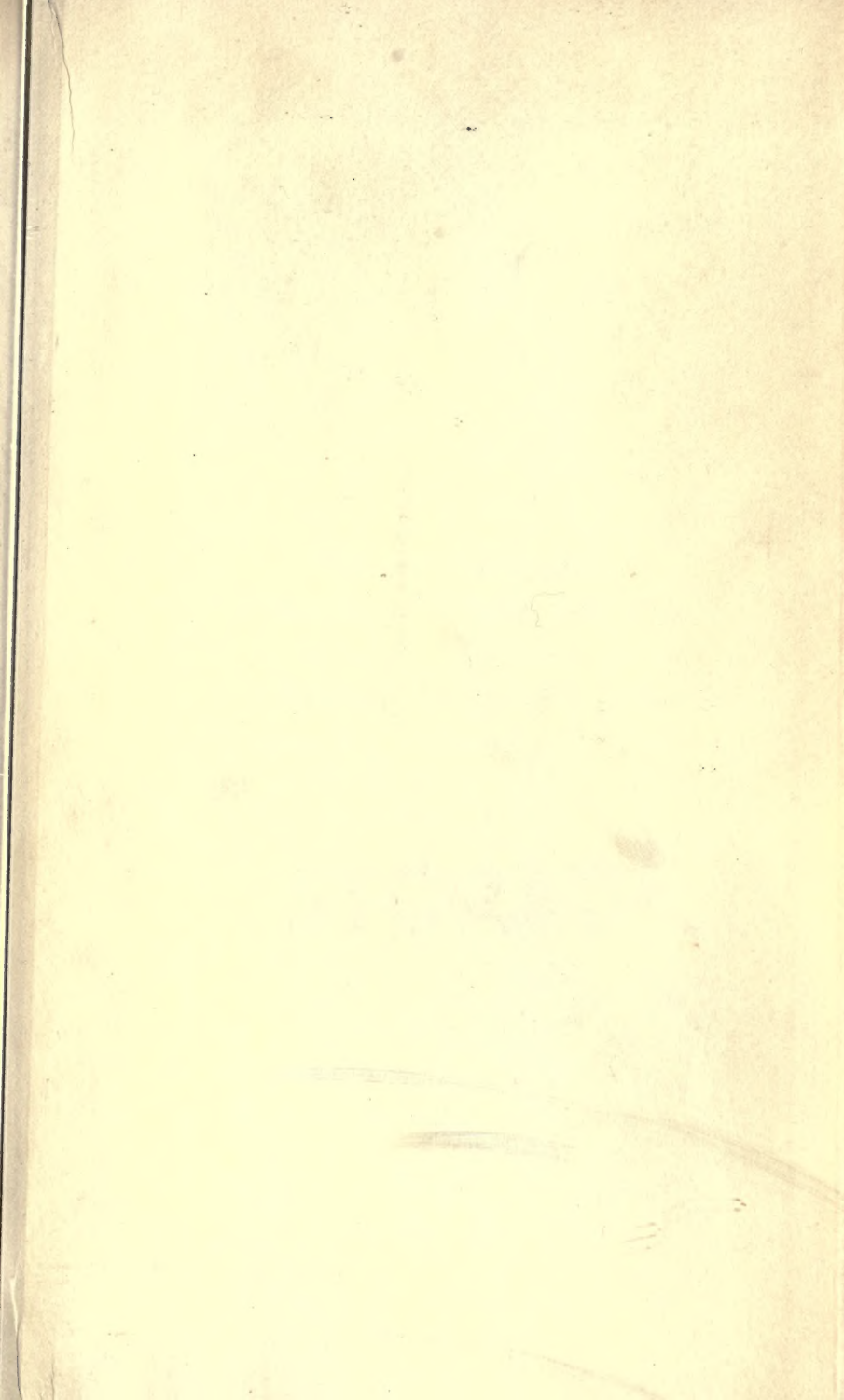
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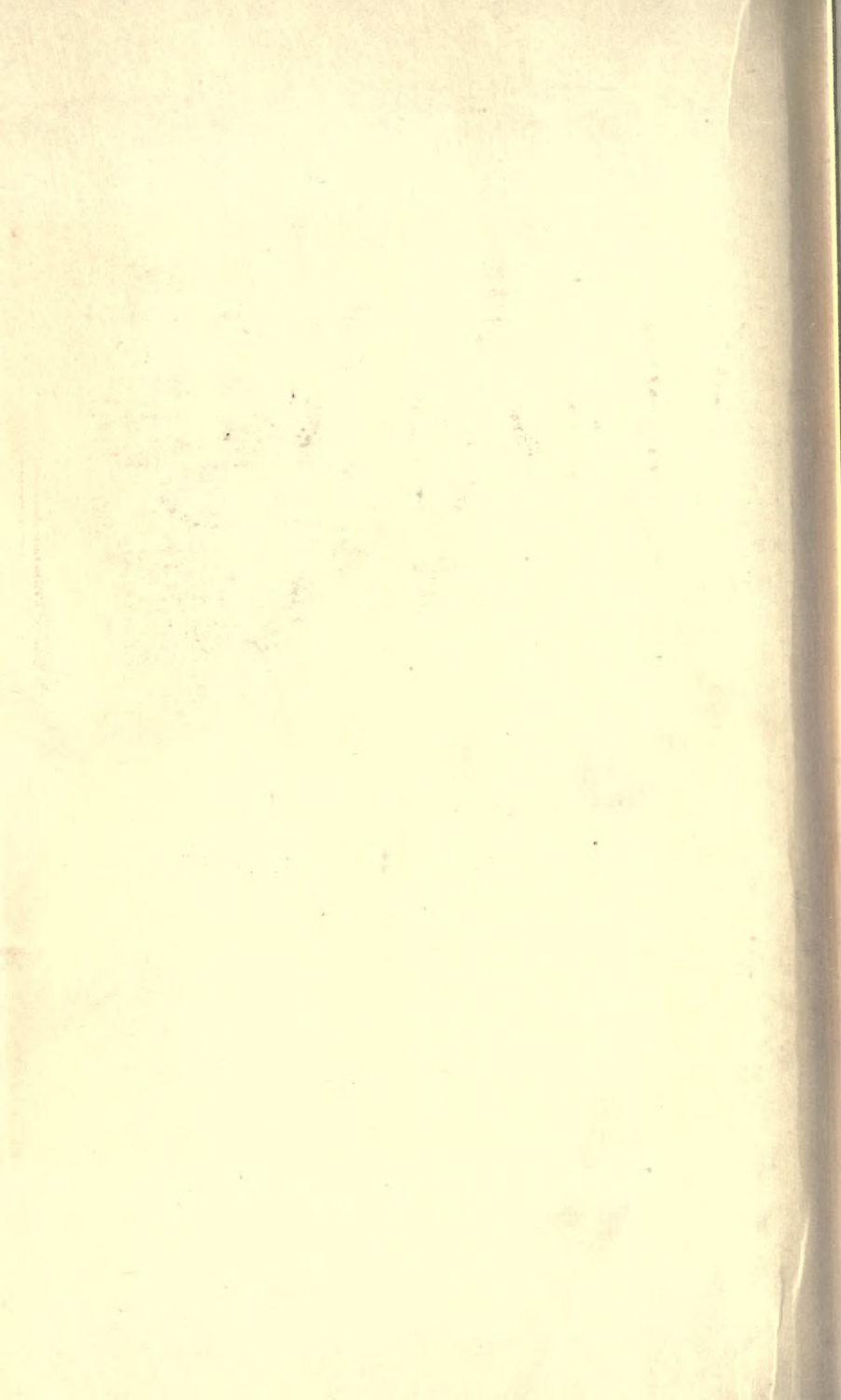












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